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NEW YORK MEDICAL JOURNAL,

A MONTHLY RECORD OF MEDICINE AND THE COLLATERAL SCIENCES.

OCTOBER, 1865.

ORIGINAL COMMUNICATIONS.

On some of the Abuses which pertain to the Employment of Bandages in treating the Stumps of Amputated Limbs, with Cases. By JOHN A. LIDELL, M.D., of New York, Professor of Anatomy in National Medical College; formerly Surgeon U. S. Vols., in charge of Stanton General Hospital; Inspector Medical and Hospital Department, Army of the Potomac, &c.

The results of the capital operations in Surgery, whether favorable or otherwise, depend very much upon the care and skill with which their after-treatment is conducted. No matter how dexterously or how ingeniously any one of the great operations may be performed, the ultimate safety of the patient, in such case, is by no means assured, unless the after-treatment be judiciously and sagaciously followed up. When viewed from this stand-point, it does not appear exactly right for the master in the surgical art to simply do an important operation, and then turn over its after-care to a tyro in the surgical art. All the trained faculties, the careful culture, and the ripe experience of even the most gifted surgeon can be advantageously employed in anticipating and warding off the hidden

damages by which the pathway of such a patient is surrounded. He is the best surgeon who not only operates well, but manages the case well in other respects; selecting the proper time to operate, in the first instance, and never afterwards losing sight of the patient till recovery is established. I do not wish to speak in a disparaging way concerning the claims of operative surgery upon our regard; but, at the same time, it seems necessary to state, in this place, my conviction, that the superior success which attends some surgeons in the management of difficult cases, is, speaking in a general way, to be attributed quite as much to the skill with which they conduct the after-treatment as to the skill with which they operate; and every additional year's experience strengthens me in this conviction.

In order to manage any important case in operative surgery aright, suitable attention must be paid to the ventilation and cleanliness of the patient's quarters, to the alimentation and medication of the patient himself, and to the care of the dressings. Now, the employment of bandages enters very largely into the matter of surgical dressings, and, on that account, it becomes a subject of importance. Furthermore, that this subject, in so far at least as the proper application of bandages to stumps is concerned, has not, on the part of some, received sufficient attention, the following cases are offered in proof :

CASE I.—*Disastrous consequences produced by bandaging a stump too tightly; death from gangrene on the ninth day after the operation.*

April 10th, 1865, a Confederate soldier, a prisoner of war, was brought in from the front to Burke's Railroad Junction, which was the depot of our army at the time of and for a while after General Lee's surrender. He had suffered amputation of the left leg at its middle, by the flap method, and complained bitterly of pain in it. On removing the dressings from the stump, the bandages were found to be *very tight*, the soft parts swelled and gangrenous, the swelling alone extending above the knee.

The next morning, April 11th, my attention was called to him by the surgeon in whose care he had been placed on arrival, Dr. Snell, of the Confederate army. The patient was in a very bad condition. The stump was much swelled and

very painful. The swelling extended not only as high as the knee, but also some distance up the thigh. The flaps were sphacelated, and the skin presented a yellowish-brown and mottled appearance nearly as high as the knee, with several large blebs, containing a dirty, yellowish colored serum scattered here and there on this surface. This incipient mortification extended as high as the tight bandages had done, but no line of separation was yet established. There was a strong odor of gangrene. He complained much of pain in the affected part, and had a good deal of constitutional disturbance in the shape of irritative fever. But little change, either general or local, had taken place since the previous day. He informed us that the leg was amputated on the 7th, at the front, where he was wounded; that the stump was bandaged very tightly, so that it gave him much distress from the outset; that the stump could not be dressed till he reached Burke's Station on the 10th, only three days afterwards, when the condition of things above described was discovered. I directed him to be removed to the Ninth Corps Hospital, which was then located in the neighborhood, as he would not bear transportation to City Point.

April 14th. He is much worse, and very low; countenance almost Hippocratic; the gangrene has extended up inner side of thigh, almost to groin; odor of gangrene very strong; tongue dry; had been treated with tinct. iodine applied locally, and opiates, stimulants and nutrients administered internally.

April 15th. Says he feels better, and is now free from pain; but he is in reality worse, as the mortification has extended.

April 16th. He died at four o'clock A. M. I did not meet with another gangrenous stump among 2,000 wounded brought to Burke's Station.

Comments.—It has been recommended by some, within the hearing of the writer, to bandage the stump tightly in military practice, in cases where amputation is performed on the field of battle, on the ground that, by so doing, the patient would be better enabled to bear transportation to the rear, and he has seen this precept followed out by at least one prominent army surgeon, but with what results it is not known, as

the patients passed directly beyond our view. The foregoing case, however, illustrates some of the hazards attending this practice.

The gangrene of this stump appears to have been produced in the same way that gangrene of a fractured limb is sometimes produced by the early application of a tight bandage—viz., by interfering with the circulation of the blood in the part. The bandages having been put on tightly, prior to the advent of swelling, there is danger that when the parts swell up through the agency of inflammatory action, the constriction may become so great as to completely arrest the flow of the blood back towards the heart, thereby putting a stop to the processes of nutrition (vitality) in the part, and causing a wet gangrene. This accident is not so likely to happen in the case of the stump of an amputated limb as in the case of a fractured limb, because the quantity of blood to be returned through the veins is very much less in the former than in the latter instance.

Again, it seems probable that tight bandaging acts injuriously upon a stump, even if the constriction is not so great as to cause mortification: firstly, because of the stagnation of the blood occasioned directly by it; and secondly, because of the deleterious effect of such stagnation of the blood upon the reparative processes in the stump. *Mr. B. Phillips, of Westminster Hospital*, has made the following statement in point:

“A custom exists in many portions of the continent of applying very tightly a bandage around the stump, for the purpose of preventing the retraction of the muscles, and a consequent exposure of the end of the bone. This system exists at La Charité, in Paris; and it is a notorious fact that the proportion of unfortunate terminations in cases of amputations is, in that hospital, very great.” *Mr. Carswell*, the pathologist, suggested to *Mr. Phillips*, “the probability of these bandages producing a congestion, which would tend to excite the development of disease.” *Vide London Medical Gazette*, vol. xiii., pp. 193, 194.

CASE II.—*A thigh stump uncomfortable because its dressing was too loosely applied.*

April 11, 1865. While walking through a warehouse, in

which a number of our wounded had been temporarily provided for at Burke's Junction, shortly before the departure of a railroad train intended to convey them to City Point, an infantry soldier, belonging to the Sixth Corps, asked me to look at his stump, stating that it felt very uncomfortable. His left thigh had been amputated at the junction of the lower with the middle third, by the circular method. He said that the operation was performed on the 6th, for gun-shot injuries inflicted that day. I found the stump itself doing remarkably well. Healthy suppuration was already established, and there was no abnormal swelling, heat, redness, or irritation of any kind whatever in it. The discomfort was due entirely to the manner in which the dressing had been applied. The bandages (rollers) had been put on so loosely as not to afford adequate support to the parts. Indeed, the dressing which I found on his stump was so loose as almost to fall off, and extended but about five inches upwards towards the groin, while his thigh was unusually long. The consequence was that the end of the stump was not held steady, and that the long muscles of the thigh were subject to spasmodic twitchings, because they were not properly supported by exterior pressure. A roller bandage, applied with moderate firmness nearly all the way from the stump-end up to the groin, so as to act both as an expulsive and supporting bandage, at once afforded him complete relief, for which he expressed much gratitude.

Comments.—In this case the bandage certainly did not perform any useful office, as it was originally applied. Indeed, the patient's condition differed but little from what it would have been if no bandage whatever had been put on the stump. This remark brings us to the question of treating amputations by leaving the stump open, that is, without any dressing whatever. I am inclined to think that this practice is advantageous in many cases where union must be effected by granulation, and where it is not necessary to move the patient. I am confident that I have seen patients recover in hospital practice by leaving their stumps open, and they were thigh stumps at that, who would not have recovered if their stumps had been kept closed up and bandaged according to the old methods. No accumulation of matter can occur in an open

stump, and in this way the danger of a fatal termination by pyæmia appears to be considerably lessened. Besides, the open stump may readily be made to heal from the bottom, and thus the occurrence of sinuses may be avoided.

But, if it be necessary to transport the patient to a distance, as it is in the practice of surgery with armies in the field, then the stumps of the amputated limb must be fixed up by the means of dressings, in such a way that they will bear the transportation. It is clearly inadmissible, in such instances, to leave the stumps open, for the jolting of the ambulance or the railway carriage, as the case may be, will inflict grievous injury upon the flaps or other tissues that have been left to cover the stump-bone, and at the same time cause a great deal of unnecessary suffering to the patient. Besides keeping the soft parts quietly in apposition, another office of the dressings, in such cases, is to hold the stump steady, and keep the soft parts in comfortable relationship with the stump-bone. For this purpose the dressing should be light, and the bandage should be applied smoothly, evenly, with moderate firmness, and to a good distance upwards towards the next articulation, in all cases wherein amputation has been performed in the continuity of a limb, and not at its point of union with the trunk, *e. g.* at the shoulder or hip joint.

Chloroform Internally a Remedy for Congestion. By A. P. MERRILL, M.D., of New York City.

No pathological condition is more common and fatal than congestion. Whether it be active or passive, sthenic or asthenic, it is this abnormal condition in certain portions of the circulatory system, this stasis of blood in certain organs and tissues, which presents itself to the physician oftenest, and which causes him the greatest embarrassment in his diagnosis and treatment. And it is this condition, more than any other, which leads to immediate death, or which becomes the foundation of inflammation, hypertrophy and ulceration, so often resulting in protracted suffering, and the final destruction of life.

Whether congestion be considered a nervous or a blood disorder, or whether originating in the one it produces the other as one of its effects, I leave to the medical schoolmen to determine; but that we have a remedy for it, whatever may be its cause and character, is a matter of importance to the profession, and of vital interest to mankind. That remedy is chloroform administered internally.

When I declare that I have found chloroform thus given to be an effective remedy for the chill of fever—which it is capable of subduing so completely as to prevent febrile reaction, and thus cut short the disease in its inception—every physician will be able to appreciate the importance of the fact, and a large number of diseased conditions of a kindred character will occur to him which must derive benefit from the same treatment. Fever, the great outlet of human life, and Asiatic cholera, one of its most obscure and fatal forms, which now threatens to involve both Europe and America in its epidemic power, will first deserve attention. Congestion, in certain of the viscera, is an inceptive morbid condition in these fatal forms of disease, and it is this and its sequences which determine the indications of treatment. These once removed, all the rest is easy.

The cognate affections to be noticed are concussion, hemorrhage, asthma, puerperal and infantile convulsions, narcotic and strychnine poisoning, cyanosis, amaurosis, dysmenorrhœa, angina, apoplexy, asphyxia, cerebral, spinal and meningeal irritation, spasm, chorea, delirium tremens, cholera morbus, hiccup, coma, colic, epilepsy, hooping-cough, insanity, hysteria, tetanus, neuralgia, paralysis, vertigo, pneumonia, rabies, congestion of the stomach, bowels and brain, hypochondriasis, gastralgia, dysentery, diarrhœa, gout, rheumatism, dropsy, seasickness, and other affections depending upon congestion, more or less, and generally of a sthenic character; to which may be added forms of disease supposed to be of an asthenic character, such as sun-stroke and anæmic congestion caused by loss of blood. In proportion as these and other maladies are dependent upon congestion, may we expect them to be benefited by chloroform internally, both by the stomach and the rectum.

The effects produced by chloroform internally, appear to be very different from those caused by inhalation. No signs of anæsthesia have been observed from its internal use, and so far as danger arises from congestion in the use of anæsthetics, chloroform internally may be considered the proper remedy. Its effects upon the system, when used internally, are, to equalize the circulation of blood, improve respiration, relieve congestion of the skin, restore and compose the mental faculties, and produce quiet sleep. Sometimes the sleep thus produced is followed by considerable nervous restlessness; but this is only temporary, and it causes no permanent inconvenience. Indeed, I have observed it only in children who have taken large doses for the relief of convulsions.

During the last thirteen years I have used the remedy effectually in many of the diseased conditions above referred to, especially in fever, pneumonia, convulsions, colic, sun-stroke, concussion, vertigo, apoplexy, asthma and delirium tremens; but, until lately, I have not met with an opportunity to prove its efficacy in pulmonary hemorrhage. That opportunity was afforded by the following

CASE.—On the 17th of August, 1865, Anthony Duffy, aged thirty-eight, a laborer in Washington Square, was seized with hemorrhage from the lungs while returning home from his work. He had been complaining all day of a sense of fullness and oppression of the chest, was unable to take his usual food, and had several bleedings from the nose. Just before his attack he drank nearly a quart of cold milk, and eat two ripe peaches. As he passed the door of my office, he was spitting blood, and he proceeded only a few steps further, when the gush of blood was profuse. He sat down upon the steps of a dwelling, and, becoming insensible, rolled upon the pavement. I found him weltering in blood, which was still streaming from his mouth and nose, obstructing his breath. Indeed, he was gasping as in the agony of death from suffocation. His eyes were open and pupils dilated, his face pale and ghastly, the capillary circulation of the surface congested, and he had convulsive movements of the limbs. His pulse was large and very slow.

I turned his face upward, and poured into his mouth, from a

vial, about two fluidrachms of chloroform, which he swallowed, and from that moment the hemorrhage ceased. Partly from exhaustion, but principally from accumulation of blood in the bronchi and stomach, his breathing was exceedingly difficult, and he remained insensible, with considerable rigidity of the limbs. In about ten minutes I gave him another drachm of chloroform, and then he was placed upon a litter, and carried a short distance to his lodgings. On his arrival there he appeared to have recovered his senses, but his breathing continued very labored, and he was unable to speak. I then administered another drachm of chloroform, which was about half an hour from the first dose, and applied sinapisms to his breast and between his shoulders. He swallowed also a little whisky and water, which caused him to vomit, ejecting from his stomach large quantities of grumous blood; but there was no sign of present hemorrhage. From this time he gradually recovered, occasionally expectorating clots of blood.

In this case sleep was not produced by the chloroform, which I have generally considered evidence that the system was fully under its influence. Probably the intensity of the attack, and the extent of the congestions, required a larger quantity than was given to effect this purpose; but I did not then know of an instance in which more than half an ounce had been given in half an hour. A few days afterwards, a gentleman, of my acquaintance, swallowed a table-spoonful at one dose for gastric congestion, with no other effect than entire relief, with several hours of quiet sleep.

On the 4th of September, 1865, Mrs. W., a married woman, aged forty-five years, was seized with paralysis of the left side, with the head drawn back, and the whole spine curved backward. She soon became insensible and speechless. Her limbs were cold, pulse 130, eyes closed, pupils dilated, and she was exceedingly restless, with a constant movement of her right hand and arm, as if attempting to grasp something in the air. I prescribed a cathartic and sinapisms, and gave her a small teaspoonful—twelve to the ounce—of chloroform. This quieted the restlessness almost instantly, and I directed the dose to be repeated as often as restlessness returned. Following out this plan, she took an ounce during the first twenty-

four hours, with the uniform effect of relieving restlessness, and sometimes inducing sleep.

Sept. 5.—Her bowels had been freely purged, she had passed a little urine, and her limbs of the paralyzed side, hitherto, in the language of her husband, “as rigid as iron,” could be slightly flexed. But her head was drawn back as firmly as before. The same treatment was continued, with the addition of a little broth and milk punch. At five P.M. she had taken six teaspoonfuls of chloroform, or half an ounce, and the fits of restlessness were more frequent and troublesome, insomuch that she had, in spite of watchfulness, fallen from the bed. The chloroform was, therefore, discontinued, and she took during the night hyoseyamus, valerian, musk, strychnia, aconite and calomel.

Sept. 6.—She had passed a very restless night; pulse 140; breathing labored; deglutition difficult; head drawn back; limbs more rigid; had been sufficiently purged. The chloroform was now resumed as before, the first dose relieving restlessness and causing sleep. During the day and night she took six doses. In the evening the left arm and leg had become quite flexible, but not much subject to her will. Head still back, and wholly insensible.

Sept. 7.—The paralysis of the limbs appeared quite relieved, but the head still drawn back; pulse 120, and more feeble; breathing quite labored; deglutition more difficult; and she had every appearance of sinking. Chloroform was given several times during the day, relieving the restlessness as before; but at night swallowing had become so difficult that the use of internal remedies was suspended, and sinapisms applied to the spine and limbs. She gradually declined, and died at seven o'clock on the morning of the 8th.

Although this case terminated unfavorably, the great power of chloroform over disordered innervation was strikingly exemplified in the treatment, and I have no doubt that the life of the patient was prolonged by its use. Certainly, it was the means of saving her much suffering; and this without disturbing the digestive functions, as opiates would have done. The cerebro-spinal symptoms, with entire insensibility and restlessness, gave sufficient evidence of the serious character of the

disease. She had suffered an attack of hemiplegia of the same side four months previously, and from which she had not fully recovered, but on that occasion the head had not been seriously affected.

Asthenopia not connected with Hypermetropia. By E. DYER, M.D., of Philadelphia.

[Read before the American Ophthalmological Society, June 14, 1865.]

There are certain cases of asthenopia which have interested me much, and I beg to call the attention of the Society to them for a few moments. In these cases there is no hypermetropia, latent or apparent. The accommodation is good. There is in a majority of cases myopia more or less marked, sometimes even as high as $\frac{1}{3}$ or $\frac{1}{2}$. The symptoms vary in some important particulars from asthenopia dependent on hypermetropia. The patient never complains of being obliged to suspend the use of the eyes while reading, from the letters becoming indistinct or the words running together, but it is either absolute pain in the eyes themselves, or a sensation so disagreeable that he at once ceases to use them. He always says that if he chose to continue he could do so, as his vision is as clear as when he commenced to read. Rest for an hour does not relieve him, and he can not recommence his work as the hypermetrope can. The pain or sensation lasts generally for several hours, often over night, and sometimes for several days. The pain is generally confined to the eyeballs, and does not affect the forehead and temples. The mind has a great influence on the length of time the eyes can be used without feeling the pain. I have known ladies who could read a long letter from an absent husband or others in whom they were particularly interested, when they could not read half a page from a book or an indifferent correspondent. The amount of use that these patients can bear is very inconstant. Sometimes they can read for half an hour, and the next day or week they can not bear the sight of a book, and dread any attempt to use the eyes. They generally let the affection run on for some months before they seek medical advice.

General fatigue of the whole body does not often affect the eyes as it does in hypermetropes, and they can use them as well after prolonged exertion as when they are not tired. Sometimes there are uterine or heart complications, but not generally. Masturbation is often acknowledged by these patients, but whether oftener than in other diseases I can not say. There is generally some intolerance of light, especially of gas light, which is not dependent on the use of the eyes. They can bear the light if excited, and can often go to the opera or a party without feeling discomfort at the time, but the next day or two they suffer the penalty. At home, when there is no excitement, they avoid the light. The pain produced by indiscretions of the above kind is almost exactly like that brought on by using the eyes. It is mostly found in persons under thirty years of age; when occurring after that age it is generally a consequent of using the eyes too soon after confinement. It is restricted almost entirely to persons of the better class of society. I have never seen it connected with strabismus convergens, but sometimes with strabismus divergens. I have seen only one case complicated with astigmatism.

The ophthalmoscopic appearances are not well marked or constant. Generally there is nothing abnormal. Occasionally it is observed, when the myopia is excessive, that the size of the staphyloma posticum does not correspond with the degree of myopia, but this is often the case when no asthenopia exists. Frequently the optic nerve is "dirty," *i. e.* not clear, and also the choroid, which is striated. These stria run towards the posterior pole of the globe, but this appearance is so common that it can not be mentioned as a characteristic of this kind of asthenopia.

From the observation of a number of these cases I was led to the conclusion that some trouble with the accommodation was generally the cause of the pain or disagreeable sensations. An emmetropic person, with an accommodation of one-fifth, ought to be able to read without fatigue at ten inches, as he would only use half his accommodation. On the other hand, a person with myopia one-tenth should read at that distance without using his accommodation at all. Still, both complain of asthenopia. It occurred to me, although the most careful

examination failed to detect any insufficiency of the internal recti, that still there might, in both cases, be a discrepancy between the power of the ciliary muscle and the angle of convergence. In other words, I concluded that there was—

1st. Some disturbance of the relative accommodation.

2d. There seemed to be a want of tone or power of the ciliary muscle for continued action.

3d. Want of mental energy, the patient having lost confidence in his power to use his eyes.

My object is to offer to your notice a course of treatment which I have used during the past two years, which I have called gymnastic exercise of the eyes. It seems to answer the three indications mentioned above. Of course, the general health of the patient must be cared for, and this treatment is only added to the general course of tonics, air, exercise, stimulants, counter-irritants, attention to the uterine functions, &c.

1st. I change the relation of the accommodation to the angle of convergence of the axes of the eyes by glasses. To the emmetrope I give convex, No. 30 or 36. With a myope of one-tenth or less, I simply correct the myopia. In the higher grades of myopia it suffices to carry the far point out to 10 or 15 inches.*

I will, for example, suppose cases of these three classes, using the same fractions in each case, for the sake of simplicity.

An emmetrope, with an accommodation of $\frac{1}{6}$, holds his book naturally at 12 inches, at which distance he uses one-half of his accommodation. The angle of convergence of the axes of vision to 12 inches is not sufficient for him to use one-half of his accommodation, and he is furnished with convex, No. 36. If he still holds his book at 12 inches, he uses only one-third of his accommodation, and the relation of the convergence of the axes of vision to the accommodation is altered, and he finds great relief. He will probably not do this, *i. e.* hold his book at 12 inches, but bring it to 9 inches, when he will, with this

* I have treated several cases of asthenopia which, from the general symptoms, seemed to belong properly to this class, but in which a slight amount of hypermetropia existed. I gave them convex, No. 36, plus the degree of hypermetropia. They all recovered rapidly.

convergence, be able to use one-half of his accommodation, which is exactly the amount used when supplied with + 36.

A myope of one-twelfth (accommodation as in the previous case, one-sixth), can not converge to twelve inches without using some of his accommodation. He is furnished with $-\frac{1}{12}$, carrying his far point to ∞ (infinity). If he then holds his book at twelve inches, he uses one-half of his accommodation, which, at once, changes the habitual relation of it to the convergence of the axes of vision.

In a myope of one-sixth (Ac. = one-sixth), the condition is similar. Furnished with $-\frac{1}{6}$, the myopia is partially relieved, and if he hold his book at 12 inches he would use no accommodation at all. But he will not do this, but from habit will hold his book nearer, say at 8 inches, where he will use one-half of his accommodation, and do it with ease. Cases of this third class are not as common as those of the first and second.

Having thus altered the relative accommodation we come to the practical use of it in fulfilling the second indication, viz.: want of tone or power of the ciliary muscle for continued action.

As before remarked, these patients rarely seek professional advice till they have suffered for several months, sometimes years, and the ciliary muscle has lost its tone. The act of accommodation is purely muscular. Why should not the ciliary muscle be strengthened by regular exercise as well as any other muscle? My course of treatment has been as follows: I order the patient never to use his eyes without the glasses prescribed, and never to use them except as I direct. Sewing, writing, music or painting, must not be attempted during the first part of the treatment. The patient must be warned that at first some inconvenience will be found, till he becomes accustomed to the glasses. He must select a book with good, clear type of medium size, and read, according to the case, 3 to 15 minutes in the morning. If no pain follows, read a minute longer at noon, and increase a minute at evening (not after half an hour before sunset). If the pain following the first reading lasts until the time of the second, the patient must not mind it, but read a minute less than he did in the morning, and a minute less

at night, if it still continues. In this way a point must be found when he can read a certain number of minutes without pain. Then let him progress a minute each day, or faster, if prudent, though it is better to go ~~too~~ slowly than too fast. When the patient gets up to 30 to 40 minutes three times a day, other use of the eyes than reading may be carefully substituted. When 60 to 70 minutes are reached, the glasses (except in the third class) may be gradually dispensed with. The patient may read at noon one-quarter of the time without them, then one-half, and so on, leaving them off gradually. The surgeon must be guided by the nature and course of the case. One minute a day seems a small matter, but if a person begins with reading one minute three times a day and increases a minute each day, at the end of a month he will be reading an hour and a half a day.

I have found it of great assistance to explain the rationale of the treatment to the patient, and, as I have before remarked, these cases rarely occurring except in the educated classes, they readily understand it, and are anxious to assist the surgeon in the treatment. For this purpose, the term gymnastic exercise of the eyes is useful. I tell them that in reading, pure muscular action is required as much as in lifting a weight; that through want of use, general debility, or some derangement of the general system, they have lost the power to exert the muscle brought into action in reading without fatigue; that they can strengthen this muscle and increase its power of endurance by regular, constant and systematic exercise, as well as any other muscles in the body.

I come now to the third indication—want of mental energy; the patient having lost confidence in this power to use his eyes. This course of treatment serves to distract the mind of the patient and restores his confidence in his ability to use his eyes. He has become discouraged; he has had the horror of blindness carefully instilled by friends and sometimes by well meaning physicians, who, not feeling quite sure of their ground, err on the safe side, and prescribe entire rest. In these cases "the safe side" is the wrong side. When the glasses are procured and the patient is assured that there is no absolute disease of the eye as revealed by the ophthalmoscope, he commences his course of treatment with hope and zeal. The mere fact that he

is told that he *must* use his eyes gives him, to a certain extent, the power to do so.

I have notes of forty cases of this peculiar kind of asthenopia treated in this manner, and in thirty-eight the results were highly satisfactory. In one of the two failures there was serious uterine disease; and in the other, the patient was obliged to leave the city before the treatment had had a fair trial.

The necessity of persevering and following directions to the letter can not be too carefully impressed upon the mind of the patient. If they are careless and do not read regularly or read too much, they will be sure to be thrown back, and have the whole ground to go over again. As these cases are always long, extending over months, it rarely happens that, from necessity or carelessness, some indiscretion is not indulged in which entirely throws them back, and they can not use the eyes more than they could when they commenced treatment. However the improvement, when this has happened, is much more rapid than at first. I will give you one or two cases showing how this may occur, and how careful both surgeon and patient must be.

Dec. 9, 1863, Miss —, æt. 27, has been in the habit of sewing almost all day for nine years. A year ago she had "some trouble" in her eyes; since then she has not been able to use them at all.

She now complains of pain in the eyes and forehead and moderate intolerance of light. Can not read ten minutes without bringing on severe pain. Has slight leucorrhœa and pain in the back. No amblyopia; no hypermetropia; no insufficiency of internal recti. Ophthalmoscopic appearances normal, except slight congestion of the optic nerve of the right side.

I ordered convex, No. 36; Syr. ferri pyrophas., veratrine (grs. x- \bar{z} i.) ointment for the forehead, warm clothing, exercise in the open air, a tablespoonful of whisky before meals, and the gymnastic exercise, commencing with five minutes three times a day.

Feb. 21st, 1864.—Patient read seventy minutes twice yesterday, and her eyes felt so well that at the third reading she thought she would see if she could not read eighty minutes. She did so, without pain or trouble of any kind, but this morn-

ing the same old sensations came on, and she was only able to read three minutes. All the previous symptoms had returned. It was not at her menstrual period. She was ordered to begin again and read only three minutes at a time, and increase.

March 30, 1864.—Up to thirty-five minutes. Continue.

April 20, 1864.—Reads an hour three times daily without trouble. Continue. To see me again if there was any more trouble.

May, 1865.—Patient has not returned, but I have heard indirectly, from a friend of her's, that she has not had any trouble from her eyes for a long time.

In this case the indiscretion was very slight, only increasing ten minutes at a single reading when she was reading seventy minutes three times daily.

In another case a patient, with general derangement of the nervous system, myopia one-eighth, (had not used her eyes for a year,) was improving rapidly, and was reading thirty minutes at a time. She undertook some sewing and worked all day. She was confined to the bed for several days, and had intense neuralgic pain in consequence, which was relieved by subcutaneous injections of morphia.

In another case, Miss B., æt. 20, consulted me Feb. 16, 1863; had not been able to read for two years. Myopia one-eighteenth. Subjective symptoms as before described. Prescribed concave — $\frac{1}{8}$, and general treatment of iron, exercise, &c. It was not until October 22 that she could read ten minutes with ease. During this time she had been obliged three times to go back to two minutes at each reading, and increase gradually.

Dec. 19, 1864.—Can use the eyes without pain. Since then have not seen the patient, but have heard from her relations that she has no more trouble.

I will mention a case whose cure I consider entirely dependent on the regular use of the eyes. Mrs. —, æt. 41, consulted me, and gave the following history: Since she was married, nineteen years ago, she has never been able to use her eyes at all. Her husband has been in the navy, and of course absent nearly all the time. Whenever she received his letters she read them, but was sometimes confined to her bed for two days in consequence, and always suffered

intense pain in the eyes and head. She has been under treatment many times without benefit. She can not read two lines without pain, which often lasts over night. She has myopia $\frac{1}{4}$ to $\frac{1}{2}$. Health rather below the standard. Has taken tonics and iron for the last ten years. Slight maceration of the pigment of the choroid. Posterior staphyloma smaller than would be expected from the amount of the myopia. I gave her Blancard's pills, a stimulating wash for the forehead, and concave one-twelfth for reading. I told her to begin with five minutes and follow the plan explained before. In eighteen days she read nineteen minutes without a particle of pain, and came to me with enthusiasm and said: "Doctor, I have read a book through; I have not read one for nineteen years." Ten days after she reported that she had on one occasion read three minutes too long, and was put back to ten minutes. Two months afterwards she was only up to fifteen minutes, having in the meantime been twice up to thirty minutes, but having fallen back through indiscretions. Since then she got up to ninety minutes, and kept there without trouble. During the treatment I changed the preparation of iron, and also gave her veratrine ointment.

In this case, I do not think the favorable result was owing to the iron, for she had been taking it for ten years, more or less, and had been taking and was taking the ferrated elix. cinch. when I first saw her.

Hints about the Nature and Treatment of the simplest form of Bright's Disease. By JOHN C. PETERS, M.D.

For the basis of this article I commence with the assumption that simple suppression of perspiration may induce the first stage of Bright's disease. It has been well said, that the important influence which the temperature and secretions of the skin exert upon the kidneys is a part of every man's personal experience; but nothing illustrates this more strikingly than the experiments which completely suspend the cutaneous excretions. Thus: Dr. Styles had the fur of a rabbit removed

and covered the skin with a coating of collodion ; in an hour or two the urine became *albuminous*. Again, Carpenter says : a partial suppression by the same means gives rise to febrile symptoms and albuminuria.

A large number of cases of Bright disease arise from simple exposure to cold and wet ; and many more occur after attacks of scarlet fever, with or without exposure to cold, although Dr. Clark says the opinion is universal that it is exposure of the surface of the body to cold air which produces the dropsy and other symptoms of Bright's disease after scarlatina, and he does not doubt that this belief is well founded in the main. But in scarlet fever, in addition to suppression of the functions of the skin, we have a blood-poisoning, while the kidneys may be found not only remarkably congested, but their secretory and tubular surfaces may become the seat of a similar vascular injection or efflorescence to that existing in the vascular rete of the skin ; and this eruption on the surfaces of the uriniferous tubes and the associated swelling and congestion of these organs may either impede, interrupt, or altogether suppress the functions of the urinary excretion, and thereby occasion an accumulation of excremential and contaminating materials in the blood. (*Copland.*) The affection of the kidneys may, in fact, transcend that which occurs on the skin in scarlet fever, and almost rival that which can so often be observed on the throat. In scarlet fever an acrid and even corrosive narcotico-alkaloidal poison, like that which exists in the rhus toxicodendron, belladonna, stramonium, turpentine or cantharides, may produce its ravages upon the blood, skin, mucous membranes, kidneys, and nervous system. But simple suppression of perspiration may also induce blood-poisoning, for large quantities of chloride of sodium, acetic and *formic* acids, urea, carbonic acid, and nitrogen gases, and other acrid substances, are naturally excreted from the healthy skin. This may be dwelt upon much more fully in another article ; at present I wish to refer almost exclusively to the retention of watery fluid and vapor which a check of perspiration induces. Lavoisier and Seguin found the average quantity of simple watery fluid lost by cutaneous perspiration during 24 hours amounted to nearly 2 lbs. avoirdupois. Dalton assumes that 25½ ounces is the nor-

mal quantity for an adult man, while laborers in gas-works have been known to lose $3\frac{1}{2}$ lbs. in weight by cutaneous and pulmonary exhalations in *one* hour; finally, Funke has produced a forced perspiration amounting to the rate of from 10 to 26 lbs. in 24 hours. Thus, a sudden check of perspiration may cause an absolute retention of many ounces or even pounds of water in the system; and it is an almost equally well known fact that a great increase in the proportion of water alone in the blood has a tendency to produce œdema. Dropsy has been induced artificially by the injection of pure water into the blood vessels; for Poiseuille and Goodfellow, who have often attempted to force water through the capillary network of an organ, found that this simple fluid injected into the arteries did not return quickly by the veins, as it does in the case of glutinous saline injections; but, on the contrary, a great part of it escaped into the tissues and produced a local dropsy. On adding albumen to the water, or employing normal serum, this infiltration did not occur, showing clearly that albumen alone, when in normal quantity, may hinder to some extent the effusion. Again, it is absolutely necessary that the albumen should be in sufficient quantity; for if the water be in excess, the fluid will still filtrate through the tissues. Hence it is evident that a watery condition of the blood, or a great diminution of its albumen, is eminently calculated to facilitate the escape of serum through the capillary vessels; in short, favor the occurrence of dropsy.

The simplest form of Bright's disease.—In this variety the kidney is merely *dropsical*, anasarca or œdematous; its tissues and tubes are simply infiltrated by a greater quantity of fluid. After death we find a large, flabby, more or less sodden, *watery* kidney; serum drops from it in large quantity when a section is made of it; its vessels are turgid with a thick red blood that has been deprived in a great measure of its serum. Goodfellow, whose description has been quoted above, is certain that the œdematous condition of the kidney *precedes* the general anasarca, and thinks that in the most recent and purest cases of Bright's disease it would not be very wide of the truth to say that they are merely cases of parenchymatous dropsy of the kidney.

This dropsical affection of the kidney is analogous to the *œdema of the lungs*, which so often occurs in uræmia. Dr. Clark has seen several cases in which the lung, when struck with the finger, trembled like a jelly, and gave the sensation of fluctuation when percussed—so great was the amount of water it contained. From exposure to cold, or from local nervous debility, or paralysis, this dropsy of the lungs may commence and reach its height in ten minutes, and produce an amount of dyspnoea that seems to threaten life; it may continue for one, two, or three hours, and then abate, if free action of the skin or kidneys takes place; or it may persist for days, or destroy life in a much shorter time; or subside and recur at the same hour the next day, or at a distant period, always presenting the same threatening aspect. I have seen it occur and prove fatal in a few hours from the patient taking a short walk on a raw, cold, wintry day. Uræmia alone can induce it, although conjoined heart disease is a more common and efficient cause.

The effects of dropsy of the kidney are comparable to those of *œdema of the parenchyma of the lungs*; there is the same pressure on the substance of the organ, and upon its vessels, tubes and nerves. A partial or complete paralysis of the renal nerves may thus be brought about, just as paralysis of the legs occurs from dropsy of the spinal theca.

Treatment.—The mildest and the most active *diaphoretics* may be useful or required in this variety. Wood truly says: They deplete from the blood vessels, and sometimes very copiously. It is not only the watery parts of the blood that are carried off under their influence, but some of the salts, and, to a certain extent, the organic constituents also. They promote absorption by their depletory influence, and, on this principle, are sometimes used very happily in *dropsy*; he has known severe dropsy to yield to this class of medicines, in which diuretics had been employed without effect. Again, he continues: It sometimes happens that the skin falls into an inactive or torpid state, and ceases to perform its functions properly; in consequence of which the blood may become impure, and various internal irritations, whether of a vascular or nervous character, may arise. Here is an obvious indication for the

use of diaphoretics, especially those of a somewhat stimulating character. Wood admits three varieties of diaphoretics, viz.:

1st. *Nauseating diaphoretics*, like tartar emetic, ipecac, sanguinaria and lobelia.

2d. *Refrigerant diaphoretics*, including citrate of potash, acetate of ammonia, &c.

3d. *Stimulating diaphoretics*, such as prickly ash, Virginia snake root, &c.

Pereira also describes several varieties, viz. :

1st. *Alkaline and saline diaphoretics*, such as the acetate and carbonate of ammonia, the alkaline citrates and tartrates, sal ammoniac, nitrate of potash, and other salts of the alkalis, all of which are frequently used to promote perspiration.

2d. *Nauseating diaphoretics*, such as tartar emetic and ipecac, which are most useful in febrile, acute congestive, or inflammatory disorders, and are preferable to the opiate diaphoretics when there is inflammation or congestion of the brain, or tendency thereto; he thinks the diaphoretic powers of ipecac to be considerably less than is commonly supposed, and that Dover's powder owes its powers of producing sweating almost exclusively to the opium it contains.

3d. *Opiate diaphoretics*, which he thinks have a remarkable tendency to produce sweating, and that in diabetes and granular disease of the kidneys, (?) Dover's powders is the best sudorific we can use, especially when conjoined with the warm bath. Opium and camphor are also mentioned as a serviceable sudorific compound.

4th. *Oleaginous and resinous diaphoretics*, such as sassafras, camphor, mezereum, guaiacum, copaiva, and the turpentine.

Beck only mentions tartar emetic, acetate of ammonia, citrate of potash, ipecac, eupatorium perfoliatum or boneset, asclepias tuberosa, and aristolochia serpentaria as diaphoretics. Ales selects dulcamara, mezereum, viola tricolor, aconite, rhus toxicodendron, rhododendron chrysanthemum, pulsatilla, and phosphorus as the most reliable sudorifics. Other authors speak of *alterative diaphoretics*, such as sassafras, mezereum, guaiac and sarsaparilla. We now pass to the consideration of the individual remedies of this class.

1. *Citrate of potash*.—Wood asserts that when the skin is hot and dry, and the circulation accelerated, there is no diaphoretic which operates more certainly and effectually; he regards it as more certain than tartar emetic as a mere sudorific; says it allays nausea and thirst with promptness, lowers the pulse, heat of skin, and induces perspiration, as well as promotes the secretion of the kidneys. When a strong sedative impression on the circulation is desired, $\frac{1}{2}$ or $\frac{1}{4}$ grain of tartar emetic may be added to each dose, or a few drops of aconite; when there are nervous symptoms, such as morbid vigilance, muscular startings, twitchings, etc., Hoffman's anodyne or sweet spirits of nitre may be combined with it. Wood thinks it greatly superior to the spiritus mindereri, both in diaphoretic power, and for calming irritability of the stomach; it is one of the most effective anti-emetic remedies, and he knows nothing equal to it in fever with a disposition to frequent vomiting. In his earlier practice he used the acetate of ammonia a great deal, but found it almost uniformly so much inferior to the citrate of potash, and so much less acceptable to the patient, that he has long ceased to give it, except in exceptional cases. It should always be made from the juice of good, fresh, sound, and very sour lemons; of which about 4 fluid ounces should be gradually saturated with bicarbonate of potash, the salt being added slowly, till all effervescence ceases. Dose, a tablespoonful every one, two, or four hours. It is doubtless as efficient as the tartrate of potash. The addition of a small quantity of syrup of orange peel renders it much more acceptable.

2. *Solution of acetate of ammonia, or spiritus mindereri*.—For some readily accountable reasons this remedy has been used far more frequently than the citrate of potash; it certainly has cheapness and disagreeableness in its favor; the former may be a sufficient excuse in hospital practice, and in countries far removed from intercourse with the tropics. It is undoubtedly an efficient remedy, especially the German preparation, which is very much stronger than the English or American formula, but is used in one or two drachm doses, instead of by the tablespoonful, as with us. It is thought to be particularly useful in the dropsies which occur after scarlet fever and

measles, although Todd has also recommended it in inflammatory dropsies. Richter found it most valuable for its diaphoretic properties; he thinks it first renders the pulse somewhat fuller and more frequent, and augments the general activity and warmth of the skin until sweat breaks out, when these symptoms decline, and coolness and relaxation occurs; when it does not act upon the skin it augments the secretions from the lungs and kidneys. Dierbach pronounces it one of the most powerful and certain means for the production of perspiration. It is sometimes given alone, but more frequently combined with 10, 15, or 20 drops of wine of antimony to each dose, or with equal quantities of wine of ipecac, or with from one to three drops of aconite, or with spirits of nitre. Drs. Swett Metcalf, Bulkley, and Jos. M. Smith treated 25 cases of Bright's disease by the hot vapor bath and spirits *mindereri* and ipecac; of these, seven recovered, twelve were relieved, one was somewhat improved, and five died. It is evidently best adapted for mild and recent attacks; still it has overcome some obstinate and chronic cases, even those occurring in elderly persons, when given in full doses, say two ounces daily of the German preparation, and followed up for several weeks. As there are few remedies which are so successful as a teaspoonful or two of this solution in sick headaches, and as it speedily puts an end to the phenomena of drunkenness in alcoholic intoxications, it may prove useful in some of the head affections of Bright's disease. When the hydræmia is somewhat lessened, Basham gives the liquor ammoniæ acetatis in a combination which he thinks both agreeable and efficacious, viz.: in conjunction with acetic acid and the muriate tincture of iron. He has reason to think this formula more effective than when the same ingredients are given separately. The spiritus *mindereri* must first be rendered acid, by the acetic acid, before the muriate tincture is added, otherwise the ammonio-chloride of iron is precipitated, and is with difficulty redissolved in an excess of acetic acid. He prefers the following prescription:

Liq. ammon. acet. - - - - 3j.

Acidi acetic dilut. - - - - gutt xx

Tinct. ferri sesquichloridi - gutt x

to be given in an ounce of water three times a day.

The Acid Elixir of Huller, or Elixir vitrioli Mysishti, has cured several cases of Bright's disease, marked by swelling of the face, hands, limbs, and abdomen, very scanty and albuminous urine, dropsy of the chest, and œdema of the lungs, with inability to lie down, and severe suffocative attacks. Dose, ten drops every hour, in urgent cases, given in wine or gin and water; or twenty to thirty drops several times a day.

Vinegar.—With a view to its refrigerant and diuretic properties, it has sometimes been used in dropsy. Dr. Gregory, of North Carolina, employed it with great success in the quantity of a pint daily. Simpson's recommendation should not be forgotten. A Dr. Beyer has treated six cases with wine vinegar alone; he gave tablespoonful doses every one or two hours; in the course of three or four days profuse general perspiration would set in, followed by three or four fluid stools per day, and very copious diuresis; recovery took place in about three weeks, and about one and a half or two quarts of vinegar were used in each case. Cases occurring after scarlet and intermittent fever have recovered under its sole use; the appetite usually improved while using it, and the remedy was generally taken readily until a cure was nearly affected, when it became distasteful; still the progress of the improvement continued unabated, and a perfect cure was accomplished without the aid of other medicines.

Lemon juice.—Frank reports six cases either permanently or temporarily cured by tablespoonful doses every two hours; all other drink and fluids were interdicted, and only white meats, bread and vegetables were allowed; from 100 to 200 lemons were consumed in about two weeks, and improvement commenced in a few days. The patients generally perspired freely at night; the urine was increased, in some cases, to two and a half or three or even six quarts per day; if constipation was present, loose stools occurred; if debilitating or even colliquative diarrhœa was present, it diminished as the action of the remedy on the skin and kidneys increased. It seemed to be especially useful when dropsy of the chest and œdema of the lungs were present; when the face, hands, feet, and limbs were bloated; when ascites was present, and the patient was unable to lie down from dropsy of the chest, and had repeated

attacks of suffocative oppression from œdema of the lungs. It probably acts in the same way as cream of tartar, citrate of potash, and acetate of ammonia, and may be most usefully employed as a common drink, when other remedies are used or required.

Bitartrate of potash, or cream of tartar.—Bennet, of Edinburg, has distinguished himself by his warm advocacy of this remedy in Bright's disease; he regards it as the most valuable of the whole class of laxatives and diuretics, and has frequently seen it produce the most powerful effects when every other had failed. He has known cream of tartar to operate after digitalis and other remedies had proved useless; sometimes, also, after it had been given without effect at an early period of the disease, it has succeeded remarkably well at a later one, which warrants our having recourse to the remedy again and again, after certain intervals, should it not act at first. He has rarely seen other diuretics succeed when repeated attempts by means of the bitartrate had failed. In full doses it acts as a refrigerant, laxative and diuretic, and, while other purgatives augment renal, hemorrhoidal and menstrual hemorrhages, this one moderates and even arrests them. Combined with jalap it forms an efficient hydragogue cathartic, which, in some forms of dropsy, and in simple anasarca particularly, is rapid and decided in its action. Stille recommends twelve grains of jalap, thoroughly triturated with thirty of cream of tartar, as a certain and not disagreeable purgative. But the resin of jalap in a pill, with one quarter or one half grain of podophyllin, is more efficient when aided by the free use of cream of tartar water, with or without juniper berries, with which it is often associated. One half ounce each of cream of tartar and bruised juniper berries, in a pint of boiling water, is one of the most efficient combinations in the passive forms of general dropsy. In the form which so often occurs as a sequela of scarlatina it is of great service, either alone or combined with tincture of digitalis.

Finally, Goodfellow prefers the compound magnesia draught of the Middlesex Hospital, viz.: magnes. carb., five or ten grains, magnes. sulph., 3i., given in peppermint water several times a day; he says he never loses a case of the recent acute form of Bright's disease. Occasionally a small quantity of tartar emetic is added.

On the Use of Arteriotomy and other Remedies in the Treatment of Puerperal Convulsions. By A. F. A. KING, M.D., of Washington, D. C.

By the term "puerperal convulsions" we mean, not any convulsion occurring during pregnancy, but only those that are dependent upon the puerperal state. It is evident that convulsions may occur at any time during gestation from the same causes as they do in the unimpregnated female, perfectly independent of, and without any connection with, the parturient state; for example, they often arise from local irritations, such as an abscess or thrombus of the labia pudenda and other sexual diseases; also from digestive derangements, blood poisons, mental emotion, &c. The ensuing remarks will therefore be restricted to those cases of eclampsia which are dependent upon the puerperal state, and which alone, properly speaking, deserve the name of puerperal convulsions.

The method of treatment that I propose to suggest for this complaint is based upon the following views in regard to its nature and causes:

1st. That puerperal convulsions are due to an abnormal excitation of certain central parts of the cerebro-spinal nervous system, caused by an increased afflux of *arterial* blood and a deficient supply of *venous* blood circulating through those centers; in other words, central nervous irritation from abnormal distribution of arterial and venous blood.

2d. That the principal cause of this disturbance of the circulation is pressure of the gravid womb upon the abdominal aorta and ascending vena cava, whereby the arterial blood is retarded in its downward current to the lower parts of the body; while the venous is retained below and hindered in its upward current to the brain.

3d. That this mechanical obstruction to the circulation is increased by constipation, tight lacing, frequent sexual intercourse during gestation, and tonicity of the muscles of the abdomen—conditions which may help to account for the spasms occurring more frequently in primipara.

I am fully aware that this theory of mechanical obstruction to the abdominal circulation being a cause of puerperal eclamp-

sia is by no means a new one; but those who have attempted to explain the rationale of its operation have only arrived at the facts, that it causes either determination of blood to the brain, or that it so interferes with the function of the kidneys as to induce uræmic intoxication. No writer has told us, however, so far as I am aware, whether this hyperæmia of the brain consists in an excess of *arterial* or of *venous* blood—a distinction which seems to me to be of the utmost practical importance; for does not physiology teach us that the one (arterial blood) is a direct *stimulant* to the brain, while the other (venous) is a most potent *sedative*?

Now we know perfectly well that excessive stimulation of a part, if long continued, soon leads to irritation, and, if still further persisted in, to inflammation and structural change; hence whenever there is a superabundance of stimulating arterial blood circulating through a healthy brain, especially if the antagonizing sedation of venous blood be diminished, that healthy brain becomes an irritated brain; and what is so significant of central nervous irritation, whether it arise from centric or eccentric causes, as irregular muscular contractions, spasms, convulsions, and where have we a more perfect illustration of the truth of these principles than in the phenomena of puerperal eclampsia? As to the second proposition, to wit: that the principle cause of this disturbance of the circulation is pressure of the gravid womb upon the blood vessels, there may be urged the objection that the convulsions do not at once cease after delivery. But this difficulty is by no means insurmountable—it may be explained in at least two ways.

From what has already been said it is clear that irritation of the brain may be one reason why the spasms continue, or there may even be true inflammation of that organ. Indeed, cases, are not wanting in which not only has there been irritation and inflammation of the brain, but examination after death has revealed that actual softening had taken place. In such cases how can we expect that the convulsions should be cured by delivery?

Again, uterine pressure being removed and no local changes having occurred in the brain, the symptoms may yet be due to structural changes in the blood vessels. For, when we remem-

ber that the veins of the lower extremities have been for a considerable time dilated with the pressure of accumulated blood, and when we consider also that the arterial blood vessels below the obstructed aorti, *not* having received their due supply of blood, may have become somewhat contracted, or at least have acquired an unnatural degree of toniciry, is it not reasonable to suppose that the little impediment that these offer to the circulation, coupled, perhaps, with a degree of excitement of the brain hardly deserving the name of irritation, may account for the continuance of convulsions after uterine pressure has been removed by delivery?

That the disturbance of the circulation which we have been considering is increased by constipation, tight lacing, muscular toniciry of the abdominal wall, and to frequent sexual intercourse, is too evident to admit of doubt, for it is plain that each of these contributes more or less to augment the pressure of the gravid womb upon the blood vessels. And now, if these views of the pathology of the disease are correct, what treatment shall we employ?

First, as a matter of course, all causes should be removed or avoided. Hence, delivery, purgation, loose lacing, and sexual abstinence become important remedies, the last two not to be overlooked.

Second—blood-letting.—In conformity with the views already expressed, it would seem, *à priori*, that if there is a remedy which we should avoid in puerperal convulsions it is bleeding from the cephalic vein, for we must recollect that by so doing we are taking *sedative venous* blood from an encephalon already *over-stimulated* with *arterial* blood, (*i. e.* we do so indirectly, by lessening the amount of blood going to the subclavian vein; we favor the descent of that from the jugular.) Venous blood is what the brain wants and what it must and will have. The very convulsion that we seek to cure is but an effort of nature to equalize the cerebral circulation by forcing venous blood to the brain; it is one of those conservative processes by which the central parts of the nervous system strive to maintain their own integrity. And does not the relaxation that immediately succeeds the convulsions show how completely the tendency to spasm is relieved by venous hyperæmia of the brain?

Nor is it until the venous congestion of the head has once more given way to the flood of arterial blood, that the spasm again returns.

Venous blood is the natural sedative narcotic for the relaxation of muscular spasm. What is it, for example, that produces the relaxation of anæsthesia, intoxication and asphyxia? It is that in each of these cases there is an excess of the sedative carbon or carbonic acid, and a diminution of the stimulant oxygen circulating through the nervous tissues,—in a word, less arterial blood, more venous. In corroboration of these views, I transcribe a short quotation from Prof. Meigs on Puerperal Convulsion, (Letter 44th, pages 665–6.) Speaking of the paroxysm itself he says :

“As soon as the diaphragm begins to grow rigid, the oxygenation of blood in the lungs is arrested ; the hurried heart propels the blackening columns into the lung capillaries, which rob them of the small remainder of oxygen, so that, when driven over into the systemic ventricle, and thence launched into the brain and cord, that deoxidized and highly carbonated blood ceases to develop the neurosity, and the patient, utterly anæsthetized, falls into the profoundest stupor, the face and hands grow darker and darker, until the cyanosis has asphyxiated her brain. This cyanosed hue, which presents to the spectator a scene and prospects truly frightful, is always hailed by me with joy, for I discover in it a sure sign of an approaching termination of the crisis.”

Again he says: “If you hold a chloroformed sponge to a woman’s mouth you will readily induce a state of anæsthesia which will stop the paroxysm, but not sooner or more safely than the cyanotic anæsthesia itself.”

According to these views, therefore, it would seem that any remedy which tends to diminish the quantity of venous blood in the brain, as bleeding from the arm, is one not to be recommended. Yet in the very face of this we know that the indisputable test of practical experience has proved beyond a doubt that venesection is one of the best of remedies. That it does relieve them in many cases can not be denied; but I see no reason why veratria or tartar emetic might not answer just as good a purpose, with the advantage of not incurring those

immense losses of blood which are unavoidable in venesection ; for the only way that we can explain the *modus operandi* of bleeding from the arm in this complaint is that it diminishes the action of the heart, and as a natural consequence lessens the amount of arterial blood pumped upwards to the brain ; at the same time the feeble ventricular contractions retard the pulmonary circulation so that respiration is impeded, and consequently the venosity of the whole bulk of the blood becomes increased.

Venesection, therefore, relieves puerperal convulsions simply by lessening the force of the heart. Why should not veratria, tartar emetic, or digitalis do the same ? These, however, are not the remedies that I desire especially to recommend. The one that I wish to suggest in particular is arteriotomy—opening the temporal artery. The gravid womb, let us recollect, is pressing upon the aorta below ; the arterial blood can not get down, but is backing up and flooding the brain and cord ; by bleeding from the artery we lessen the whole amount of arterial blood above the obstruction, deplete the brain locally, and indirectly diminish the force of the heart quite as readily as by bleeding from the arm ; moreover, we have not robbed the brain of *venous* blood. At the same time, by opening a vein in the leg (below the obstruction)—which is the second remedy I would suggest—we relieve the distension of the lower veins, obviate congestion of the kidneys, and invite the arterial blood to descend.

In conclusion, therefore, while it must be admitted that bleeding from the cephalic vein is undoubtedly a useful remedy in this disease, it appears to me that the modified forms of blood-letting and other remedies, to which I have alluded, might accomplish better results much more promptly and with a far less expenditure of blood.

PROCEEDINGS OF SOCIETIES.
NEW YORK PATHOLOGICAL SOCIETY.

Stated Meeting, April 12, 1865.

Dr. GURDON BUCK, President, in the Chair.

STRUMOUS DISEASE OF KNEE-JOINT—DR. L. VOSS.

Dr. Voss presented the right knee-joint of a girl, seven or eight years of age, who, for some time past, had been suffering from strumous disease of that part. She first presented herself to the doctor with a large abscess on the outer aspect of the joint. This was opened. Subsequently sloughing took place in the abscess, and the patient ran down so rapidly that exsection could not be performed. The thigh was accordingly amputated, to save life.

On examination of the joint, after the removal of the limb, a most interesting condition of the part was seen to exist. The outer condyles of the joint were very much roughened and diseased, while the inner condyles were perfectly smooth and uninjured. These two parts were separated from each other by a vertical false membrane, and the partition thus formed seemed to be complete enough to prevent the extension of the disease further than the affected part.

DISEASE OF THE HIP-JOINT—DR. VOSS.

Dr. Voss stated that he had another specimen in the hands of Dr. Jacobi, which he hoped to have an opportunity for showing that evening, and would do so as soon as that gentleman arrived. The specimen was one of disease of the hip-joint, in a very advanced stage. The child had been treated for four or five weeks in the horizontal position, by extension, when it began to show symptoms of tubercular meningitis, by which it was carried off.

Dr. Voss remarked, in this connection, that two years ago he had occasion to show the Society an inflamed hip-joint which had been removed from a child who died from the same cerebral disease. In that instance the joint showed the existence of a small sequestrum in the substance of the neck of the bone. With respect to this tubercular meningitis, subsequent to inflammatory affections of the joint, Dr. Voss stated that he could recollect four or five similar cases, and he thought that the relation of the brain trouble to the disease of the joint deserved careful study.

Dr. SAYRE remarked, that the specimen was a very interesting one,

as illustrating the beautiful attempt on the part of nature to limit the disease. He had never seen such a membrane in that relation before. He had often noticed the frequency with which the external condyle is affected, compared with the internal, and it was due to this fact that the peculiar deformity, the subluxation of the knee backward and outward, took place. He had met with two instances of death by tubercular meningitis following strumous disease of the joint, and agreed with Dr. Voss in the conclusion that the cerebral disease was not of unfrequent occurrence, under the circumstances.

Dr. BUCK also recalled an instance of death from meningitis occurring in a child eight or nine years of age, in the advanced stage of hip-joint disease.

ULCERATION OF THE COLON—DR. LEWIS SMITH.

Dr. LEWIS SMITH presented a portion of the descending colon, taken from a child aged two years and four months. The sickness of the patient dated back to last summer, when it had the summer complaint, and the bowels continued relaxed from that time until death. The evacuations were not very frequent—three or four in the course of twenty-four hours—never bloody, and usually of a dark color. There was no marked emaciation attending the symptoms, and the patient, not being considered in any particular danger, had not been subjected to any treatment. At the commencement of this month it was suddenly seized with a convulsion, and died in the course of six hours after.

At the autopsy no permission was granted to examine the brain. The small intestines were healthy, and the mesenteric glands were not enlarged; the seat of the abdominal disease was found to be in the colon. The mucous membrane of this canal was thickened, and there were numerous minute ulcers, especially in the descending portion, which extended simply through the mucous membrane, and were surrounded by a more or less complete zone of vascularity. In the centre of some of these ulcers were occasionally found papillæ, which appeared to be the solitary follicles.

The case, aside from its pathology, was interesting in reference to the production of the disease. The child lived on the ground floor in a wooden tenement house, which was surrounded on three sides by stagnant water, and the disease of the intestine seemed to have been induced by the insalubrity of the situation, and in that respect, as well as in its pathology, it bore a resemblance to the diarrhœa which has been so common among our soldiers.

ILEUM IN PURPURA HEMORRHAGICA—DR. SMITH.

Dr. SMITH also presented the ileum taken from a child eight years old. The patient was of healthy parentage, and was in good condition until the 25th of last month, when it began to vomit blood, and soon after to discharge blood from the bowels. On the following day, spots of purpura hemorrhagica were observed upon the lower extremities. From this time until the 31st of last month, when death ensued from exhaustion, the patient had from three to four bloody passages a day.

The thoracic and abdominal viscera were both examined, and nothing unusual was noticed, except a bleached appearance of the organs. The liver was very fatty. Dr. Smith had never seen such an extreme fatty degeneration of the liver before in a child so young. There was no lesion of the intestine noticeable, save a great vascularity of the mucous membrane of the ileum, and this was evidently the source of the hemorrhage.

The case seemed to be one of purpura hemorrhagica, without any pre-existing disease. So far as his experience went, Dr. Smith considered it a rare case.

URINARY ORGANS IN A CASE OF VESICAL CALCULUS—DR. GURDON BUCK.

Dr. BUCK exhibited a specimen of the urinary organs removed from a patient who died the day previous, in the New York Hospital. The patient was admitted March 14th, for stone in the bladder, and was treated by lithotripsy. He was a deaf mute, a native and resident of Bergen, N. J.; of spare habit, light complexion, and sixty-four years of age. The history that was obtained from him was, that he had been suffering from symptoms of stone for four years. The information that could be gotten, considering his difficulty in communicating his ideas, was necessarily very imperfect. No member of his family accompanied him, and his wife, who frequently visited him, was also a deaf mute. He was very much disturbed by frequent micturition. On examination, the stone was felt without any difficulty. His general condition did not appear materially to have suffered, for he kept up his active habits of a farmer without any serious inconvenience. The urethra was found of full dimensions, and there was no appreciable enlargement of the prostate, as far as could be judged by the usual method of examination. The instruments entered the bladder without occasioning, in the examination, any undue amount of irritation. The stone was found to be about the size of an English walnut.

From the size of the stone, from the favorable condition of the urethra, and the general aspect of the patient, it was considered to be a case favorable for the operation of lithotrity. The first operation was performed March 16th. The bladder was first injected with about five ounces of water, and the stone was then readily seized by a grasp of about an inch, and was crushed. There was some delay in making a subsequent seizure, and further attempts were for the time accordingly desisted from.

On the 30th, the operation was repeated. The effect of these operations was not particularly disturbing; there was no febrile action developed; but, as usual, there was some blood mixed with the urine after the first twenty-four hours, together with an increase of the viscid, tenacious mucous secretion, which, before the operation, had been quite abundant.

The same quantity of water was injected into the bladder at the second operation. The instrument entered readily; the lithotrite passed the neck of the bladder with entire ease, and there were five seizures promptly made, and in each instance the fragments were crushed. Not more than from three to five minutes were occupied in the operation. On the 27th, a third operation was performed; four seizures were made with good effect, and no more time occupied than during the previous manipulations. On the 31st, a few days subsequently, a fourth operation was performed. The patient was rather of a desponding tone of mind, feeling lonely, in not being able to communicate with those around him. About this time he would have a slight febrile reaction almost every day; but, aside from this, he seemed to be about the same, until the 1st of April, when it was noticed that his appetite began to fail, and he gradually sank, and died on the 4th.

Dr. BUCK was disappointed in the case, in the small discharge of the fragments resulting from the operations. These operations were successful in the breaking up of the stone, and he was positive that a considerable number of the fragments had been for some reason retained. The explanation for this, however, was found in the extensive disease of the urinary organs at the post-mortem examination.

A careful examination of the interior of the bladder clearly proved that no injury whatever was inflicted upon the mucous membrane of that organ by the instrument. The bladder is of large size, and the fundus was found rather thin, the mucous membrane at the same time being softened. There were numerous fragments found in the cavity of the organ. The left lateral lobe of the prostate was enlarged in-

ternally; which enlargement, by virtue of its position, prevented the passage of urine; while, at the same time acting as a valve, it did not offer any impediment to the entrance of an instrument into the bladder. In consequence of this damming up of the urine, the bladder was increased in size, and the ureters very much distended. The kidneys, being about the natural size, were filled with abscesses, some of which contained calculi.

Dr. Voss asked if he had been in the habit of using anæsthetics in cases of lithotrixy.

Dr. BUCK stated that he preferred that the patient should have his sensibility, in order that the action of the bladder might be controlled. He stated that in a second case of operation for lithotrixy, which was performed upon a lad seventeen years of age, he was, on account of the irritability of the patient, compelled to use ether. The repetitions of the operation numbered fourteen or fifteen, and in every one, with two exceptions, ether was employed.

Stated Meeting, April 26, 1865.

DEEP-SEATED ABSCESSSES—DR. GURDON BUCK.

Dr. BUCK cited the following case of deep-seated abscess, then in progress at the New York Hospital:

The patient, aged twenty-five, was admitted a few days ago into the New York Hospital, with an inflammatory swelling of the neck, on the left side, occupying the lower portion, including the region of the sterno-cleido mastoid muscle, and extending three fingers upwards from the clavicle, and spreading outwards perhaps two fingers' breadth beyond the outer margin of the clavicular region of the sterno-cleido mastoid muscle. The swelling was diffuse, not salient, and its surface was red, tense, hard, and resisting. The integuments and subcutaneous tissues were thickened, and did not admit of any slipping upon each other. The patient suffered a good deal of pain, and had great difficulty in moving his head in any direction, on account of the disturbance in the neck caused by it. He complained of trouble at times in his breathing, at night, and also in his deglutition. The swelling was of recent origin, and evidently of an inflammatory character; it had existed a certain number of days, long enough for suppuration to have taken place, though no distinct fluctuation could be felt.

From the complaint in his breathing and swallowing, I was led to explore his throat, particularly the entrance of the larynx, and ascer-

tained, by touch, that on the corresponding side half of the edge of the epiglottis was thickened, and the aryteno-epiglottidean fold which constitutes the margin of the entrance of the larynx was also thickened. This condition of things put a very serious aspect upon the case, and gave ground for the belief that, if allowed to continue, it might at any moment produce sudden suffocation. I regarded it as exceedingly important to procure an outlet for the matter. Though there was no fluctuation to guide me, yet I could recognize one spot where the surface yielded more than elsewhere, and that determined me as to the point for explorative puncture. I carefully considered the situation of this spot in reference to the vessels; it was to the outside of the course of the external jugular vein, and sufficiently above the clavicle to be above the course of the arteries which occupied that region. I made an incision through the skin to the depth of about a half or three-quarters of an inch, and then made use of a director, pressed it through the tissues, and presently arrived at a place where no further resistance was met with. This indicated that I had reached the cavity of the abscess, and by a little forcible stretching of the opening a drop of matter flowed along the groove. I then carefully enlarged it, giving exit to the contents of the abscess.

There was no further increase of the symptoms which gave us solicitude; the patient was conscious of relief, in a greater facility in swallowing. In the progress of the disease, the matter showed a disposition to approach the surface at the median line, above the notch of the sternum, where the parts softened and where evidently the communication was more direct with the matter. This directed me to make a free opening at that point, and since then the case has gone on steadily and well, and the swelling in the epiglottis and neighboring parts has disappeared.

In connection with the foregoing case, he then related others which resembled it.

Some years ago, a patient of the New York Hospital, who had a deep abscess in the neck, died instantaneously while in the water-closet. On examination, after death, there was found œdema of the glottis, and this had been produced from the extension of the inflammation from the abscess, which at the time was in a sloughing condition.

These abscesses, situated deeply and behind the fascia of the neck, are very serious in their nature, and expose the patient to great danger. The same is true of deep-seated abscesses higher up.

In my private practice, an office patient presented himself with a

deep abscess under the angle of the jaw, and was beginning to suffer in the same way as in the first case related. Though no fluctuation was perceptible, I pursued the same course, carefully puncturing and enlarging the abscess, taking care at the same time to keep clear of the vessels in the neighborhood.

My belief is, that the parts intervening between the surface of these abscesses and their cavities are generally exterior to the course of the arteries. The facial artery, which hugs the edge of the jaw, passes over to the anterior insertion of the masseter muscle, and is not pushed to the surface with the swollen parts. I think, as a general rule, this is the case; so if you are careful not to go beyond the cavity of the abscess, in your puncture, the operation is not attended with danger.

There is another case which I might relate, which was an instructive one. It was an instance of a deep abscess in the iliac fossa.

It had been treated in the medical wards of the New York Hospital, where the existence of the abscess was recognized. While under treatment there the abscess opened into the bowel, and a sudden discharge of pus took place per rectum. The matter continued to show itself afterwards in the stools. When transferred to the surgical side, his condition was this: In the right iliac region there was a swelling filling up the iliac fossa as far as the brim of the pelvis, so that you could not insert your finger between the crest of the ileum and the abdominal wall; in other words, it was impossible to make a depression in the integument at that point equal to that which it is even possible to make in the fleshiest subject. This swelling filled up the depression seen at that point, and could be traced a certain distance towards the median line, when it was gradually lost. It extended down to Poupart's ligament, and distended its outer half. The integument overlying the tumor was neither red nor adherent. The thigh was slightly retracted, and was kept in that position, whether the patient was standing or in the recumbent position. The swelling had extended upwards, and overlapped nearly one-half of the crest of the ileum posteriorly. This portion of the swelling was resonant, and, on handling it, you could distinguish that it contained air. This was explained, on the supposition that there was a communication with the gut, and that gas had escaped into the cavity of the abscess. The course I pursued with this was, to carefully make an opening over the posterior portion of the swelling. I cut down carefully, and satisfied myself that I could transmit an impulse from the portion of the swelling occupying the fossa, anteriorly to this posterior portion which was

resonant. The question was, whether the gut had not made its way to the surface; but the transmission of the fluctuation from one portion of the swelling to the other settled the question that such was not the case. An opening was made at the point indicated, when a free escape of pus and gas took place. An interesting point in the progress of the case was, that pus no longer discharged itself from the bowels, except immediately after the operation, when the matter appeared in the stools two or three times in succession. The opening is now closing, and, it being the fourth day, the patient is doing well, and has no further passages of pus per rectum.

This deep-seated abscess, of which the previous case is an illustration, has for many years interested my attention, and perhaps my observations have directed the attention of surgeons in New York more particularly to the subject. I published an article on the subject some years ago. It is an abscess which may originate in the iliac fossa or in the lumbar region, but it is important to determine that point in reference to the treatment which should be employed. I have met with it in both sexes. In two cases which I can distinctly recall, it was preceded by erysipelas of the limb affecting the absorbents. These latter were both males. The youngest case that I have met with was in a female, a girl of tubercular diathesis, and highly scrofulous. In one case the abscess was produced by a foreign body which had made its way from the cæcum, and set up inflammation posteriorly in the fossa of that side. The foreign body afterwards presented itself in an opening in the groin which had previously been made for the evacuation of matter, and proved to be a pin, which was extensively incrustated. That patient, a male, ultimately recovered.

In several of the cases the abscess has originated in the puerperal state. The proper treatment, which has been successful in my hands, has been this: upon ascertaining the existence of the abscess, I, without even waiting for fluctuation to establish itself, make an outlet for the matter below the outer half of Poupart's ligament, and parallel with it, first through the integument, exposing the fascia of the thigh. This fascia being divided, you pass under the ligament and get behind the iliac fascia. The incision thus made is to be kept open by a plug of lint, renewed every twenty-four hours.

Dr. DRAPER remarked, that the last case referred to by Dr. Buck had been under his charge at the hospital. There was resonance down to Poupart's ligament, before the abscess discharged itself, and Dr. Draper was inclined to think, at the time, that it was due to adhesion of the walls of the intestine at that point. He was under the

impression that the man had suffered a great deal from pain in the back, and for that reason he thought that the diagnosis of disease of the spine had been made, and that the abscess had been referred to that condition as the cause. He had one case very similar to that, about two months ago. A woman, forty years of age, complained of severe pain in the middle of the inner aspect of the thigh, which at first was supposed to be due to muscular rheumatism; but external applications failing to have any good effect, a more careful examination was made, when deep-seated tenderness in the groin was discovered. That tenderness increased, and a decided swelling took place; an abscess, eventually formed, pointed below the inner half of Poupart's ligament, and was opened by Dr. Sands. There was marked retraction of the thigh, as one of the symptoms. That patient entirely recovered.

Dr. SANDS inquired, in what proportion were abscesses which pointed in the groin due to diseased bone in the spine? It was evident to him that there were cases where dead bone was not the cause, as was shown by the patients recovering.

Dr. BUCK remarked, that he should not regard the encountering of bare bone in the tract of the abscess as necessarily a condition which precluded a recovery. I can recall, said he, two instances—one in a young man of scrofulous constitution, who had a well marked abscess in the iliac fossa—which I treated in the way described. It continued to discharge for nearly two years, during which time I made every effort to close it, but failed. I was able to pass in a bougie some seven or eight inches, and could distinctly feel it grate over a bare surface in some portion of its tract. The patient finally recovered.

Another case, which I might here refer to, was one of hydrocele, occupying the inguinal canal. It was treated with free incision, and afterwards was succeeded by a large abscess in the hollow of the limb, which was also opened, and remained discharging for some time. In that case, too, I could pass a bougie seven or eight inches over bare bone. This case also recovered.

Dr. BUCK finally mentioned the following case: A sailor worked on board of a vessel up to the day of his admission into the hospital, when he presented himself with a prominent swelling that occupied the iliac region, hugging the crest of the ileum, and extending close down to the outer portion of Poupart's ligament. It stood out quite salient. There was no tenderness, no redness, no adhesion of the skin; neither was there any embarrassment in the motions of the limb of that side. The case excited a good deal of interest, and at a consulta-

tion there was some diversity of opinion; some were disposed to regard it as one of those deceptive cases of fluctuation; and, in fact, that was the view entertained by the gentleman who had charge of the case. In one of my examinations I made this discovery, that fluctuation could be transmitted from the swelling to below the lower half of Poupart's ligament. That discovery cleared up the case to my mind, and convinced me not only that there was fluid there, but that that fluid was behind the iliac fascia, and in the iliac fossa. The case was proceeded with, however, with the view of removing a tumor. An incision was made across the most prominent portion of the swelling, the skin and fascia were in turn divided, when finally the tumor was opened into and discharged a *large quantity of pus*. I then requested my colleague to introduce his finger and see if it would pass behind the outer half of Poupart's ligament. That decided the situation of the abscess, and confirmed the conclusion arrived at. This case was a remarkable one, as showing an entire absence of the inflammatory element; and there was no disease in the lumbar region, as it was impossible for such a state of things to exist and the man all the time be engaged in the laborious occupation of rolling barrels of flour.

AMERICAN OPHTHALMOLOGICAL SOCIETY.

Second Annual Meeting, Wednesday, June 14, 1865.

MORNING SESSION.

DR. E. DELAFIELD, President.

The business in order was the topic of discussion selected last year, viz.: The morbid conditions, functional and organic, included under the name of asthenopia, their pathology and treatment.

Dr. E. DYER, of Philadelphia, opened the discussion by reading a paper, (see page 11 of this number.)

DISCUSSION ON ASTHENOPIA.

Dr. NOYES.—I should like to make the inquiry of Dr. Dyer, if, when he has brought these patients up to what he considers a fair amount of reading power—that is, for example, half an hour at a time—he permits them to dispense with their glasses?

Dr. DYER.—I do not permit them to dispense with the use of the glasses until I have got them up to an hour and half three times a day, which is fair use of the eyes; then I allow them to drop them gradually:

for example, one session during the day, generally at mid-day, to leave off their glasses and read a quarter of the time, and see if they can do it; and then, if they can, a little longer next time, till they can entirely dispense with them.

The President (Dr. DELAFIELD).—If the Society will indulge me, this is a subject of which, for forty years, I have seen so much that I feel as if I must say something upon it. There is now living a lady, married in 1812, wife of a classmate of mine, at Yale College, who, in consequence of this disease, has never read or written during all this time. Within three months I have had as patient a lady, eighty years of age, with this same effect. The common case, as you all know, is with young people, though no age is exempt from it; no period of time will allow a case to get well spontaneously. I believe I can say I have hardly known a case to get well without medical treatment. Every case that has come to me has been told, sometimes by medical friends, sometimes by others, “rest your eyes and you will get well.” I never saw a case get well by resting the eyes. This difficulty occurs very largely, as you all know, in girls educated at boarding schools; very largely in girls more or less hysterical, at all events more or less delicate, from imperfect and improper physical management, improper diet, want of exercise, want of fresh air. The symptoms are very like: it is true there is a variety, but I do not think the variety is material; I find very much the same treatment adapted to all. For instance, as a general rule, the sight, so to speak, is perfect; the patient looks at an object perfectly well. I have not found that a large proportion of them were myopic—a small proportion in my practice; and I see very much of this particular disease. I have seen and treated many hundreds of them within the last forty years. In a moderate proportion of cases, after the patient uses the eyes a certain length of time “the eyesight blurs,” as they use the phrase; they do not see as well. This is an exception to the general rule. I do not find it necessary in treating the case to remark the difference. I did not intend to have said any thing on this subject, and therefore have not arranged my thoughts—so my remarks will be somewhat desultory.

The principle stated by Dr. Dyer is mainly the correct one; I have been in the habit of applying it for many years. Some of you who know what kind of practice I have had in the city are aware that I see a great many women with diseases peculiar to their sex. My attention was first brought to this particular mode of treatment by the fact that so large a portion of the women coming to me with uterine diseases had been told, “you must keep still.” They have kept still

for months, some of them for years, and have got no better. They come to consider quietude their only safety. I saw, this morning, a patient with moderate hemorrhoids; I can not get her to get off her sofa; the consequence is, she suffers all the time. Well, there is a whole class of diseases of the female constitution aggravated by indulgence; that is, they make an attempt to do a particular thing which causes pain, and finding that the pain occurs, they feel it will be wrong to pursue it. Now, in cases, for instance, of ordinary ulceration of the *os uteri*, the practice general in our profession is, to keep these patients quiet; and they will hardly ever get well if they are kept so. I say to these patients, "you ought to walk about," or "you *must* walk about; go on from day to day, and by and by you will acquire the power;" and after a short time such patients go on horseback, and take any amount of active exercise, notwithstanding the ulceration of the *os uteri*. They get well. Such cases I am in the habit of seeing from necessity, and I treat them in this way as a general rule. I remember at this moment a lady brought to me from Milwaukee, who had been four years on her couch with one of these affections, thinking she could not walk. In two months she rode on horseback, and in three months went to Europe, by adopting this principle and attending to her general health. Of course, this principle alone was not sufficient. Many years ago, in the Dublin Hospital Reports, the application of alkaloids, those which have an irritating effect on the skin, was recommended, and it was taken up, not by the profession, but by an empiric of this city, and his success in the treatment of this disease by these alkaloids was very remarkable; but the profession did not take up this particular class of remedies. That class of remedies, conjoined with the principle that Dr. Dyer has been elucidating, and attention to the patient's general health, I think I may say, will cure forty-nine cases out of fifty of this troublesome disease. I hardly hesitate in saying to a patient coming to me, "I think you will get well in the course of a few weeks." The care of the patient's general health, however, is the foundation of every thing. All these patients are nervous, sensitive; as a general thing all of them have been in the habit of reading and writing. Few of them have been in the habit of taking the amount of fresh air and exercise necessary. This want of exercise and management of the general health very much originates, it appears to me, from the fact that our profession do not go sufficiently into detail, in telling patients, for instance, every thing they may eat, and every thing they must avoid; every thing that goes into their mouths must be regulated; also the precise amount of exercise, exposure, and fresh

air. We are so much in the habit of saying, "you must carefully avoid indigestible food"—that amounts to just nothing. If we go accurately and carefully into detail in giving our patients directions on this subject they will get the benefit of them and derive advantage; and so in regard to the hygienic directions generally. All this I consider very essential to the foundation of the treatment; but the application of the alkaloids is the remedy. In the first place, I give the patient a lecture as to the management of her general health, after catechising her very carefully as to all her different habits. Then I at once rub upon her temples and forehead, and perhaps behind her ears or the nape of the neck, say, the saturated tincture of aconite, or a solution of *veratria*, gr. iv., *ad aquam*, ʒi.; and I say to the patient, "Now, when you go home see how much you can read. You certainly can read fifteen minutes the first time." And, under the influence of this article, (where it is producing a powerful influence,) she can. As a general rule it is so after the application of one of these powerful stimulants. Then the practice is to make, every day, one of these efforts. Saturated tincture of aconite is one stimulant. The next day I would apply a moderately strong solution of two grains of *veratria* to the *drachm*, applied in the same manner, the same way. Then I apply something like Granville's Lotion. I use equal parts of aqua ammonia and alcohol—say two ounces of such a mixture. That is applied for thirty seconds to the forehead, temples, back of the neck, behind the ears, always stopping short of vesication. Every other day some one of these things is done. I find, as a general rule, if the patient in the mean time will have followed my directions as to general health, she can, in the course of a fortnight perhaps read half an hour. Poor patients—infirmity patients—can not be gotten well in this way. I do not find that prescribing these things—telling patients about these things—answers any purpose. I have often received letters and answered them, prescribing what should be done, but relief has not been afforded. The surgeon himself must see to it. It is troublesome to the patient; she neglects the applications and fails to observe all the directions; and I know of no other way of producing the desired effect than for the physician to attend to it himself. The only other mode in which these applications in very obstinate cases are employed, is by a solution of *veratria* applied to the eye itself. It is very painful. To a grain of *veratria* put an ounce of water. I never went beyond a grain. I used to employ it for obstinate cases of *asthenopia*, but lately I have not found it necessary; I can get along without it. I know my friends in this city—and I have often talked with them on the

subject—have not seemed to attach as much importance to this class of remedies as I do. I no longer feel any doubt about the cure of these cases; and, although these remedies may not be infallible, their effect is so good that in using them I have never failed.

I suppose I might talk all day, gentlemen, upon this subject, but I wont tire you. These are the main ideas which I wish to throw out upon the subject. The exercise of the eyes, as insisted on by Dr. Dyer, I look upon as called for in these cases—the forced use of the eyes, not stopping simply because there is pain. By the way, I remember a curious fact which occurred a good many years ago. A gentleman who had this disease told me he determined not to be conquered by it, and he went out into the bright sun, and let the sunlight fall upon the book, and then read in spite of the distress it gave him; he said it did him good, and he got better. This very thoroughly illustrates the principle Dr. Dyer insists upon.

Dr DERBY (of Boston).—I have but a few hastily prepared remarks to offer on the classification of asthenopic affections, and the relief of one of the more important.

Understanding by asthenopia the inability to sustain the consecutive use of the eyes on near objects, the vastness of the field opened for discussion becomes at once apparent. And, owing to the variety of structural anomalies, as well as of local or general morbid processes that may bring about a state of things coming, to some extent, under the above definition, the necessity of classification makes itself at once felt.

It was originally proposed to divide asthenopia into two great classes :

1st. Where the continued use of the eyes on near objects is physi-
cally—

2d. Where the same is optically, impossible.

The classical description of the *first* by Donders* is familiar to all. I give it in his own words : “ All work on near objects causes either pain or an extremely disagreeable sensation in the eyes, sometimes, too, redness, blinking with the eye-lids, a disposition to close the eyes, etc., so that it becomes necessary to cease from work. The symptoms then generally diminish. Sometimes they gradually assume a more permanent character and then become very obstinate. Vision is of natural acuteness. The range of accommodation remains normal, even though the eyes be wearied and painful. Not infrequently

* Archiv für Ophthalmologie, Bd 4, Abth 1, S 330.

a small amount of myopia is present. Convex glasses are of no use. I can give but unsatisfactory hypotheses as to the nature of this affection. Nor am I inclined to believe that hyperæsthesia of the retina lies at the bottom of it."

In this class of cases, then, the eyes can not be used on near objects on account of the pain and inconvenience resulting. It will be observed, however, that the vision, up to the last moment of the act, remains as distinct as at first. In the *second* class the symptoms are so familiar that it seems superfluous to recall them. Indistinctness of vision arising after a more or less continued effort, accompanied by pains in the forehead, a flow of tears and a disposition to close the eyes, all of which phenomena are relieved by rest.

Into one or the other of these classes every case of asthenopia may be brought; the *first* as little understood in our day as when Donders first wrote in 1858, and when firmly fixed as little amenable to treatment, an *ignis fatuus* among ophthalmic affections, in one case inherited, in another the result of imprudence, often absent in the invalid and present along with the most vigorous health—a complete enigma as to its seat, its cause, or its cure—more common, I think, in this country than in Europe, and, as far as my observation reaches, more commonly met with in the East than in the West, decreasing in frequency of occurrence as we withdraw from the Atlantic seaboard. The *second*, generally congenital, and reducible to one or two causes—removal of the boundaries of the field of accommodation from the eye, or insufficient muscular action; in other words, hypermetropia or insufficiency of the internal recti.

I have carefully collated the cases of asthenopia occurring in my own private practice, excluding infirmity patients as offering relatively fewer cases of the affection, and less opportunity for subsequent study. Out of 1,800 recorded cases in general ophthalmic practice I find 369 cases of asthenopia in general. Of these 241 belonged to the first class and 125 to the second, or asthenopia simplex. Nearly all of these latter were dependent on hypermetropia.

I am aware that two years after his views on the subject of asthenopia were broached, the system of classification above mentioned was given up by Donders, who adopted the second division as the type of the affection and insisted on hypermetropia as almost invariably the cause. He says,* "I readily admit that many different conditions were included under the name of hebetudo or asthenopia.

* On the Anomalies of Accommodation and Refraction of the Eye. London, 1864.

When inconvenience was felt on continued exertion, this appeared to some sufficient to justify the inference that asthenopia existed. On this account different forms of irritation, congestion in myopic eyes, hyperæsthesia of the eye, with increasing pain on exertion, different affections of the retina and of the choroid, may, even the beginning of trachoma, and foreign bodies in the sac of the conjunctiva, might all be united under one denomination. But I can not concur in the adoption of such a primitive semeiotic method. It leads inevitably to confusion of ideas and of conditions."

Undoubtedly the causes here cited give rise to symptoms more or less resembling those of true asthenopia. But, abstracting entirely from such and all of them, it must, I think, be admitted that in our country and climate there exists a widely diffused and exceedingly prevalent affection—generally to be traced either to excessive use of the eye in health, or to the premature use of it during the convalescence from some debilitating disease of the general system, where continued use of the eyes on near objects is impossible without present pain and subsequent discomfort. The eye is, to all appearance, both externally and ophthalmoscopically, absolutely normal. Paralysis of the accommodation fails to discover a vestige of hypermetropia. The interni are of normal strength. The general health may be satisfactory—often indeed, I admit, is not. Sometimes, under the influence of an alcoholic stimulus, the symptoms temporarily disappear, or under the excitement of important business the eyes are used with apparent ease, and, when the excitement ceases completely relapse into their old condition. This affection may last weeks, months, or a lifetime, and in the majority of cases resists any and every therapeutic agent. That it is excessively prevalent among us I think none will deny, and what to call it except asthenopia, or its synonym hebetudo, I am at a loss to conceive. The term "morbid sensibility of the retina" has justly been relinquished—an inconvenient name, which in itself begged a controverted question.

A single word, in conclusion, on the subject of hypermetropia. This great cause of a leading form of asthenopia of course occurs as such in every conceivable degree. I do not remember to have found trouble arising from a less degree than $\frac{1}{70}$, while out of more than 120 cases, six were found where $H = \frac{1}{7}$, and three where $H = \frac{1}{6}$. Of course acquired hypermetropia is here excluded. Some curious instances of inherited hypermetropia came under my observation. The two Messrs. R., one possessing $H = \frac{1}{12}$, the other also hypermetropic in a high degree, married wives each of whom had $H = \frac{1}{10}$. Of the issue of the first

couple, three daughters consulted me on account of asthenopia, and were found to have $Hm \frac{1}{30}$ to $Hm \frac{1}{36}$. Two children of the second couple have respectively $\frac{1}{10}$ and $\frac{1}{6}$.

As is well known, the original method of treating asthenopia arising from this cause was by neutralizing the manifest hypermetropia, and knowing the amount of latent that lay concealed, to keep pace with its development from time to time until all became manifest. And much stress was laid on the supposed necessity of making the neutralizing glass as far as possible part and parcel of the eye, only to be laid aside when the eye "ceases to see." These views have recently been subjected to important modifications, and the assumption that moderate use of the accommodation on distant objects can not be indulged in without injury, has been proved fallacious. Considering, therefore, the inconveniences arising from the constant use of glasses for all purposes, and the state of dependence on them to which the wearer becomes finally reduced, we may allow a patient who sees distant objects moderately well without assistance, to content himself, temporarily at any rate, with the use of glasses for the near.

Dr. WILLIAMS (of Cincinnati).—The experience which I have had for a number of years with this class of cases has been considerable, and I have found a number of patients where I had, by careful and repeated examinations, excluded all idea of the usual causes of asthenopia, such as myopia, hypermetropia, astigmatism, and paralysis of accommodation, and still found cases that, notwithstanding the sight was perfect, the range of accommodation normal, and the eyes ophthalmoscopically and in every other respect perfectly natural, still the patients were not able to use their eyes for any length of time without feeling great inconvenience. But I have generally found in these cases that before coming to me they have consulted physicians frequently, and have been invariably told that their symptoms were those of approaching amaurosis—and the patients having a horror of that, think they must not use their eyes. Thus they regard the non-use of their eyes as an absolute necessity; and go on using them but very little, feeling always a terrible apprehension that they will eventually become blind. Of course their eyes, under such circumstances, do not get any better. I believe that in all these cases the disease is seriously aggravated by the mental apprehensions of the patients; and I make it a point in their treatment to make a careful and thorough examination, so as to be able to satisfy myself whether there is any disease that is likely to destroy or seriously impair the vision. If there is not, I assure the patient, in the most confident way possible,

that he need not have any apprehensions about being blind. I usually tell him he could not get blind if he were to try. I endeavor to get the confidence of my patients in that way, so as to relieve them of the apprehension. I know, from my own individual case, the effect of relief of that apprehension. Years ago, as a student, I was in the habit of using my eyes a good deal at night, frequently reading till one or two o'clock in the morning—sometimes in my bed for several hours; and, after persisting in this for several years, although I had remarkably strong eyes, they began to have symptoms of asthenopia—fatigue, pain, and sometimes an indistinctness of vision, produced, no doubt, by the temporary congestion of the choroid; and I can now, by looking at a white surface at this time, see the choroidal circulation upon my eyes. When I was at the clinic of Dr. Graefe I was troubled in that way, and was greatly apprehensive that I might eventually lose my eyesight, although it was still perfect. I asked him to examine my eyes with the ophthalmoscope. He did so, and told me that there was no appearance of disease; that there was no congestion of the retina; no organic lesions of the retina or choroid; it was just a little nervous irritation. (I don't remember the exact explanation he gave.) He distinctly assured me I need not be apprehensive of losing my eyes; and from that day to this they have been better without my having done any thing, except just to use my eyes a little more reasonably than I did before that time. I have had this experience occur to me in the treatment of patients time and again. I remember one lady who had not read five lines of print, at one time, for twenty years. She had been told by all the physicians she had ever consulted that it was absolutely necessary for her to refrain from using her eyes; she would finally become amaurotic, and the less she used the eyes the longer the serious catastrophe would be delayed. As she was a little presbyopic, I told her she must use glasses. I had the greatest possible difficulty in getting her to look for a second through them. I told her the doctors who had treated her before had not understood the case; I was satisfied the glasses would cure her; and I fitted her with a pair of Number 30, convex, which relieved the presbyopia, and she has been using her eyes just as much as she pleased up to this time, and with very little inconvenience. In the treatment of those cases which I have found in young men and young ladies, I have sometimes resorted to convex glasses, according to the directions of Dr. Dyer, without exactly knowing why. I have tried this mode in two cases—one a young lady, twenty-eight years of age, and the other a young man, twenty-two or twenty-three. I made

them use their eyes with convex glasses whenever they began to read; and I insist on the regular, systematic use of the eyes in all these cases, always stopping short of fatigue, if possible; and by the use of moderately convex glasses I have found benefit, although there was no *hypermetropia*, and no paralysis of accommodation. I use first as a sort of *anodyne*, a solution of sulphate of morphine. I use six grains to the ounce, a pure article, which generally produces but little if any irritation. I use it three or four times a day, put three or four drops in the eye, and give it time to take effect. Patients have experienced very decided relief from it. In addition to that I give them iron, or quinine, or *nux vomica*, or something of the kind. I give them regular, systematic exercise, and use the local application of cold water in the form of the *douche* to the eyes twice a day. I apply it with a small spout, the end being perforated; put it on the hydrant and let it *douche* their eyes for five or ten minutes; and I have sometimes had them relieved of great neuralgic pain; but the rule governing the application must be the strength of the eyes and the power to persist in using them. With these remedies the physician may begin with a strong assurance that the patients will not lose their eyes; I have generally succeeded in relieving them.

Dr. DELAFIELD.—I should like to mention, in respect to this forced use of the eyes, that they should never be used before breakfast, and never when the patient is much fatigued. This is a very essential point to be observed.

Dr. DYER.—My own experience has satisfied me of the necessity of observing this.

Dr. WILLIAMS (of Boston).—I did not intend to say any thing on this subject, but perhaps it may be well that all the corroborative experience should be brought to bear that we can bring with reference to this peculiar class of asthenopic cases; and all my own experience leads me to believe, with Dr. Dyer, in the importance of the systematic gymnastic exercise of the eye, as he terms it, and in the importance of making a good impression on the mind of the patient. My attention was very early drawn to these cases; before, in fact, I had any thing to do with them myself. I was so situated that I was constantly hearing about cases of successful treatment of what was called morbid sensibility of the retina. Patients had been for a greater or less length of time suffering under that affliction, which is asthenopia, as Dr. Dyer has described it; and they had been subjected to rather an active treatment, and generally told to absolutely refrain from using the eyes at the same time. Local depletion had been used

pretty extensively, and low diet insisted on. Many of these patients came to the charlatan to whom you have alluded, and he put into their eyes remedies which had a perfectly surprising influence both on the mind and body. The patients were put to bed about twenty-four hours, after putting strong alkaloids on the eyes, and the impression made was astonishing. The patients considered the man a prophet, and ascribed the cure, which was in many cases effected, as coming through these remedies which he and no one else knew how to use. What were the real facts of the case? This man, who had a great deal of tact, took these hysterical patients and gave them roast beef and stimulants, let them take drives about the country and enjoy themselves as much as possible, taking away all their old associations. (Whether he directed them to use the eyes or not I can not say.) He placed them in circumstances different from those in which they had been placed, and he altered absolutely the general treatment. His patients did extremely well, and the observations I had of some of them, who afterwards came under my care, was, that he ascribed much more importance to the general treatment than to the local applications, which were of this agonizing character, and which, I believe, were used more to make an impression on the patient than on the disease. It answered the purpose in making an impression on the disease, however, by acting through the minds of the patients. Having this first brought very prominently to my notice, and observing the cases as they began to come under my own care, I was satisfied that his patients were, many of them, suffering from dread that they were to be blind. Feeling an inconvenience in using their eyes, and feeling they would "strain the eyes," as they termed it, and grow gradually blind, they followed readily enough the advice given them, and that was to refrain absolutely from using the eyes, sometimes with the advice to shut themselves up in a dark room. On examination with the ophthalmoscope, I did not often find the slightest tendency to amaurosis; and I encouraged the patients to use their eyes all they possibly could. I told them there was no tendency whatever to blindness, and they could use the eyes to advantage rather than leave them entirely without exercise; that they needed exercise for developing the eyes as much as any other organ, and that to be developed they should be kept in constant use. I found it important at the same time to give them tonic general treatment, and especially to caution them, as many of them were persons with delicate organizations and health, against an imprudent amount of exercise. I told them to take exercise regularly and frequently, and to divide the ex-

ercise, not taking so much at one time as to become thoroughly prostrated; for it seemed to me that the general nervous prostration which ensued on excessive exercise prevented the eyes from getting along well. I agree with the observations already made as to the use of the eyes before eating. It is also very desirable that patients should not use them when suffering with illness, or at the menstrual period. I found out, before knowing much about hypermetropia, that glasses, in a certain number of cases, answered very well. I mean that glasses were of more use than merely to give them encouragement in using the eyes. I gave them glasses which I tested carefully, and fitted to the eyes as well as I could, and they went on better; but in a considerable number of cases these were only valuable where hypermetropia existed, as we now recognize it. The patients were required to use the eyes by encouraging them, and by giving them tonic general treatment, and placing them in circumstances cheerful and hopeful; so that, instead of having the attention constantly fixed on the idea of blindness, and filled with morbid fear of it, they were encouraged to believe that they never would be blind; they might use the eyes some, and gradually more and more.

AFTERNOON SESSION.

The President, Dr. DELAFIELD, in the Chair.

Dr. SANDS.—If this discussion of the question of asthenopia is still in order I will make one or two remarks, although I do not suppose I can say much to elucidate this difficult subject. But my experience has included several cases that come under the second category mentioned by Dr. Derby, one of which interested me very much on account of its obscurity and the difficulty I had in treating it. The case was that of a young gentleman, about seventeen years of age, living in New England. He had been sent abroad by his father to complete his education, and to spend a couple of years in Europe. He was at that time in the enjoyment of perfect health. Having made the tour of the Rhine and spent two or three months in Switzerland and the neighboring country, he returned to Paris, and then began to experience pain in using his eyes. He had never been a very hard student, and was not over-working his eyes at the time. He was exceedingly surprised at the occurrence of the symptom mentioned, and went to a well known oculist in Paris, who, upon examination, told him he was suffering from hyperæsthesia of the retina, and that the affection had been brought on by the previous habit of masturbation.

tion, which, I think, he had indulged in up to that time, though never to any great extent. The physician created an immense amount of alarm in the mind of the boy, and placed him under treatment of a complicated character, a great many remedies being prescribed, both externally and internally. He was enjoined to relinquish the use of his eyes, and under no circumstances to apply himself to work in less than a year. This intelligence was communicated to his parents at home, who received it with a great deal of disappointment, and sent for him to return. Upon his arrival, I was consulted. I had supposed that perhaps the asthenopic symptoms might depend upon the existence of hypermetropia. On examination, however, I detected a very slight degree of *myopia*—I think, one-fortieth; the power of accommodation equaled $\frac{1}{4}$, and vision under no circumstances was indistinct. He did not have the blur of which hyperopic patients, who are suffering from asthenopia, usually complain. He referred the pain to the ball of the eye rather than to the supra-orbital region. I could discover no evidence of disease by ophthalmoscopic examination, except a small amount of capillary hyperæmia in the optic nerve. The sharpness of vision was normal; I believe it was a little above the normal, for he saw No. 20 of Dyer's test type at a distance greater than twenty feet. This young man's physical health was good; his mental condition, however, was one of depression, induced, I think, by the advice given him when he was under the care of the oculist in Paris. I did all I could to encourage him, and assured him that he would recover. I was satisfied that he had no disease of the fundus, and no insufficiency of the internal recti. Having read an account of some cases by Donders where asthenopic symptoms were ascribed to inordinate contraction of the ciliary muscle, I was inclined to suspect that this might possibly be the cause of my patient's symptom. He suffered no pain when looking at distant objects, but only when the ciliary muscle was brought into play during the act of accommodation. I provided him with convex glasses of twenty inches focal distance. I hoped that by this means he would be able to relax the greater part of his accommodation, and in this way get rid of his painful symptoms. But when the optic lines were made to converge upon near objects, it was about as painful for him to suspend his accommodation with the use of the convex glasses as it had previously been to exert it without the glasses. Donders relates several cases in which asthenopic symptoms depended upon spasmodic contraction of the ciliary muscle, and in which complete relief followed the instillation of a solution of atropine sufficiently strong to wholly paralyze the accommodation and

set the ciliary muscle at rest. It was found necessary, in one instance, to maintain the action of atropine for a period of six months before a cure could be effected. Meanwhile, the patient was allowed to read by means of convex glasses of suitable strength to compensate for the temporary loss of accommodation. These facts, I think, would lead us to look for an explanation of asthenopic symptoms often rather in the apparatus of accommodation, in the ciliary muscle and the nerves which supply it, than in any error of refraction or any disturbance relating to the general health. I have not yet tried the effect of atropine in the case I have related, as the young man is no longer under my observation.

Dr. NOYES.—The discussion in which we have been engaged has been to me personally very interesting, and doubtless to those of us who have to meet cases of asthenopia the explanation of these puzzling cases, or any light thrown upon their character, will be the information which we are most desirous of receiving. The discussion has thus far related to only one class of asthenopia. I think it will not be amiss cursorily to run over some of these diseases which, in the general classification of medical men, are included under this name of asthenopia. Turning this morning to the definitions of asthenopia as they occur in some of our recent standard books, in an edition of McKenzie, the last published in this country, I found his definition of the term to be, "Incapability of sustaining the eye in adjustment for near objects." Lawrence, without exactly defining the term in a formal manner, gives these words, which I take as his definition: "An affection of the retina from excessive employment, commonly called weakness of sight." McKenzie grouping the symptoms round adjustment to near objects, and Lawrence grouping the symptoms round simply a morbid condition of the retina. Turning then to the treatise of *Stellwag von Carion*, his definition of asthenopia (and he has an admirable practice of formally defining the disease which he is about to discuss) includes :

First, "The inability to keep the dioptric system or the visual lines for a long period directed to near objects; and, *secondly*, in close pathological relation to this condition hyperæsthesia of the retina and ciliary nerves."

This definition of asthenopia is unquestionably the definition which we would be most likely to adopt; but at the same time I think there is a great tendency to exclude from the definition of asthenopia all those cases of refractive errors and muscular disturbances which are not properly disturbances of the ciliary muscle, and to include under the

term of asthenopia, or *hebetudo visus*, cases in which neuralgic symptoms predominate. This is the definition which the term is gradually assuming.

But a glance at the literature of the profession shows that such was formerly not the understanding of asthenopia, and we all know perfectly well that there are now at least two or three well recognized subdivisions into which it can be divided. First, errors of refraction principally, or hypermetropia and astigmatism. Second, insufficient power of the internal recti muscles: they can not perform their work of converging the visual lines when the eyes are engaged upon near objects. Again, thirdly, there is another set of cases characterized by excessive irritability of the retina, and accompanied by ciliary disturbance, or neuralgia of the eye. They suffer from extreme intolerance of light; and on examination by the ophthalmoscope, as far as it can be permitted, will discover no lesion, no organic change; and that there can be no morbid change is corroborated by the fact that their visual power attains the normal standard. It is not for me to say any thing on the subject of hypermetropia, or astigmatism, or myopia; it would be a piece of supererogation. I only desire to recall these facts, in the form of historical reference. Then, as to the insufficiency of the internal recti muscles. This unquestionably has a nearer relation to the subject of asthenopia, in the aspect in which we are now disposed to view it, than do the simple errors of refraction; because this muscular part requires to be exerted in all use of the eye upon near objects. The accommodation of the eye and this muscular convergence are inseparably connected. We are all aware that the correction of this difficulty is to be accomplished in one of three ways: either by the division of the *external recti* muscles, by the use of prisms, or, what has been a very recent suggestion, the employment of electricity, acting directly on the internal recti muscles. Of this last I know nothing, with the exception of what is contained in the last number of the *Archives für Ophthalmologie*, by Dr. Landsberg. As to the division of the external recti muscles, I have performed it in only a very few instances, and that only in cases where the amount of insufficiency was so very decided as to require a prism of fourteen or sixteen degrees to enable the patient to see near objects, and in these cases the results are sufficiently satisfactory. As regards the employment of prisms, and keeping them constantly in use, my experience has not been very large. I have never ventured to employ prisms whose angles were higher than an aggregate of eight degrees. The dissatisfaction which patients expressed was sufficient to prevent me from push-

ing the experiment with higher degrees. With prisms of four and six degrees I have found decided benefit. I have found that where the deficiency was confined to but one internal rectus, the correcting prism must, as it were, be divided, and one-half be placed before each eye.

I have been compelled, as I mentioned to some of the gentlemen here in New York, some time ago, to look for a readier method of discovering insufficiency of the recti interni than has hitherto been practicable. The means proposed by Graefe, viz., by looking at a vertical line with a round spot on its centre, was the foundation of the examination; and I discovered that I succeeded better in taking a piece of black paper, and drawing a white line and dot on it. Across the middle of the line, and at right angles to it, I draw a dotted scale, divided into eighths of an inch, and numbered each way from the centre. The white dot is at the intersection of the vertical line and the horizontal scale. As usual, a prism of about 10° is placed with its base upwards or downwards, before one eye, and a disk of red glass before the other. If now there be insufficiency of the interni, the patient sees two lines as well as two dots; one of these lines decidedly red, and the other nearly white, although fringed by chromatic aberration. The difference in color is enough to attract his attention much more quickly than by looking at a black line upon a white surface. The scale, moreover, enables the patient to describe the degree of separation. I have, with this diagram, saved myself the loss of much time. I could never before easily get patients to understand what I wanted them to do.

Dr. Dyer states that he prescribes weak convex glasses to asthenopic patients, with normal refraction, for the purpose of altering their *relative* accommodation. In other words, not for the purpose of diminishing the amount of accommodative effort by the aid of the convex glass—for example, subtracting $\frac{1}{36}$ from $\frac{1}{12}$ —but to cause their accommodation to act under a different angle of convergence. He calls into play the intimate association subsisting between the internal recti muscles and the ciliary muscles. The convex glass brings the near point a little nearer to the eye, and requires a little greater effort of the internal recti. In this way, perhaps, a good influence is exerted upon the ciliary muscles. They may, perhaps, be said to have a “better purchase” in their contraction, while the systematic training develops their strength.

I have seen benefit derived from wearing weak prisms, in cases where I did not feel certain that the internal recti were at fault, but

in whom asthenopia was sufficiently distressing. I have combined a prism of two degrees with a convex lens $\frac{3}{8}$, and thus relieved a very troublesome degree of asthenopia. With these contrivances, there occurs a change in the relative accommodation. Certainly not the change produced by simple convex glasses; but yet the eyes accommodate, under conditions unlike those to which they have been accustomed. I have, in some cases, doubted whether the benefit derived from prisms was because of the relief to the internal recti, and not rather because of the modification in the conditions of accommodation. Before leaving the subject of the use of prisms, I would remark, that some patients, using prisms of only two degrees each, have complained of the spherical aberration. A physician, accustomed to use the microscope, and a draughtsman, both asserted that the page comes up to them in a convex surface. After my attention was drawn to the phenomenon, I myself became conscious of it. I think it curious, and am not prepared to suggest an explanation. It is not strictly spherical aberration, but is a loss of flatness of field, a sort of pseudoscopy.

If you will permit me to have a word to say in regard to cases to which I think the term of hyperæsthesia of the retina may properly belong, the subject may be illustrated by three cases which I distinctly remember. Two of these were in the persons of physicians. One of them, a surgeon in the navy, had been stationed on the African coast; had been subjected to the intense light of the tropics. His eyes became extremely sensitive to light, and he not only could not use them, but was rarely free from ciliary neuralgia. He had subjected himself to all sorts of treatment, almost poisoned himself with strychnine, had used quinine, adopted all the local applications which his knowledge and study of what was written on the subject could suggest. After he had been suffering in this manner for about five years, he came under my observation, and, on examination, I saw there was no departure from a normal structure, and no deficiency in the power of accommodation, or of any other function. I could assure him that he was in no danger of becoming blind; that he would probably, ere long, be better; advised him as to a course of out of door exercise, and avoidance of the use of his eyes and of bright light, and gave him blue glasses. Under this encouragement and general management, after being on shore six months, away from the irritating influences to which he had been subjected, he recovered the use of his eyes and again went to sea. There is now living in the city a physician who has the same misfortune. He came to me for a certificate, to be presented at the provost marshal's office. I made an ophthalmoscopic

examination, with as little gas-light as I could get along with, and it produced agonizing pain for twenty-four or forty-eight hours. It was always the case that the attempt to read, or to fix his eyes upon distant objects, as well as near, provoked extreme pain. There was no error of refraction, impairment of vision, or muscular disturbance; the general health fully up to the average. I could regard this case in no other light than one of extreme irritability, whose starting point was the retina, and which was reflected upon the ciliary muscle and nerves. One other case, of a similar sort, has presented itself to my observation. These are the several categories which present themselves under the title *asthenopia*. They will hereafter be known by their proper designations of errors of refraction, debility of the internal recti, *hyperæsthesia retinæ*. The term *asthenopia* is at the present time, when used to designate a distinct disease, and not merely a symptom, being narrowed down to cases of ciliary spasm, or *neuralgia*.

Dr. ALTHOF.—I have not much to say in addition to what these gentlemen have very ably exposed to us. I have only to remark that so far as the treatment of cases of *asthenopia* by glasses is concerned, it is a subject which is becoming from day to day more important, and I have no doubt that if we had the same facilities in the city of New York that are had in Europe for providing our patients with glasses, we could use them to very great advantage in many cases of *asthenopia* where now we can not. So far as this graduated table which Dr. Noyes has prepared and exhibited is concerned, I have used a similar contrivance for a time, but have not found any great advantage from it. It is not easy to be used while examining a patient for insufficiency of the internal recti. I am suffering from a very high degree of this difficulty in the right eye; there is a divergence of two and a half lines. I am utterly unable to produce the double images. With the aid of the colored glass which has been used for a good length of time—which I have seen used as far back as 1858—I have always been able to bring those persons that are not very much accustomed to giving correct statements about their eyes, to something like a decent statement respecting them. I have heard, this morning, in regard to *asthenopia*, very little that I have not before read in the books of the great authors. The only thing I have heard which was entirely new to me has been contained in the remarks of Dr. Dyer—that portion relating particularly to the use of concave glasses. The suggestion of Dr. Dyer, as to the employment of a kind of gymnastic exercise of the eye, has for some time been known to the profession, but the

exactitude with which he forces his patients to do exactly what he wants them to in observing his directions about their eyes is entirely new to me. I have never been able to do it, and I am pleased to learn that there is a way of reaching this result—a thing which, as I stated at a meeting of the New York Ophthalmological Society, has puzzled me extremely. It is very difficult, as I have said, to find out whether or not people are really suffering from insufficiency of the internal recti. I myself belong to this class, and if I were examined by a very skillful oculist, I think he would be puzzled to know whether I did or not—to know that I am squinting divergently to a very considerable extent; that I have all the symptoms of asthenopia of the internal recti without being able to produce the double images which ought to accompany the squinting. Dr. Dyer has stated that by giving persons concave glasses which enabled them to read at about fifteen inches, he has been able to relieve a good deal of suffering. I should like to know what method Dr. Dyer has adopted to satisfy himself that in these cases there certainly was no insufficiency of internal recti. The treatment of insufficiency of the internal recti by concave glasses is a thing which Graefe has exposed in that first paper of his, if I am not mistaken, in which he speaks of the three different classes of asthenopia—alludes to the certain classes of asthenopia to be ascribed to insufficiency of the internal recti; and, particularly lately, a good many cases have come under my observation in which I have only been able to satisfy myself that this insufficiency of internal recti existed after very long and careful examination—three or four of them. I would be very much obliged to Dr. Dyer if he would tell me if his examination has satisfied him beyond any doubt whatever that the patients in these cases, in which he relieved them by giving them convex glasses, were not suffering from insufficiency of the internal recti. As to the suggestion of Dr. Sands, to apply a convex glass to an emmetropic eye, it would be only substituting one evil for another, and I can not agree with him. It is an old experience, and has been admitted by all the authors who have so far advanced our knowledge in the structure of accommodation and refraction. They never, under any circumstances whatever, are able entirely to paralyze their accommodation. A man who has presbyopia, who, instead of reading at eight inches reads at twelve, you give him glasses one twenty-fourth and you will never find that he reads at eight inches; he will read at nine or nine and a half inches. So far I am sure; Dr. Dyer gives his emmetropic eyes a convex glass; they all do what he wants them to do—that is, they exercise a certain range of their accommodation; they do not really read in one

point, but they do really use a certain fraction of the whole width of their accommodation. So far as the using of concave glasses for myopic eyes is concerned, I should like to have Dr. Dyer answer whether there was any insufficiency in all these cases.

Dr. DYER.—I am very happy indeed to answer this question. I have no particular method of determining whether there is any insufficiency of the internal recti besides those commonly used. I am never satisfied with the first examination; I always try both eyes successively and come to the best conclusion I can; if I can not determine it, I take it for granted that it is not there, although I am perfectly aware of the difficulty which Dr. Noyes and Dr. Althof have expressed—the difficulty of determining when the person is not particularly educated or bright. The point that I wished to bring before the society, and the one that I wished particularly to have discussed—an idea which I am not competent to explain, though I have been working on it in giving glasses to these people—is this: that there is a want, a deficiency, of the relative accommodation, or a discrepancy between the power of the internal recti and the ciliary muscle; and if that is altered, and the hypermetropic man has a convex glass given him, of course the range is altered. But it is the same with the myope. That is one principle I have been working on, and endeavoring to establish a more perfect relation between the two.

Dr. ALTHOF.—Mr. President, I was far from believing that Dr. Dyer had not taken every pains to ascertain the real state of the eye muscles in the cases of myopic patients to whom he gave concave glasses. As I stated before, I am very glad indeed to have heard the point introduced in the way it has been to the notice of the society, for I am very glad of having the opportunity of trying some remedy for cases which I have given up as incurable by me. As to the allusion made by Dr. Noyes to the article by Dr. Landsberg in the last number of the *Archives*, I for my part do not believe that any very beneficial results can be hoped for from the use of electricity. It is at least nine years since a physician of Berlin, one of our most experienced physiologists, communicated to the society of Berlin his ideas on the use of electricity in diseases of the eye. I recollect myself having seen in his house cases sent by Graefe, and I have seen any quantity of cases treated by electricity in Berlin; I have seen plenty of them in Vienna under the care of the now rather celebrated electrician, Dr. —, but I have never seen any benefit whatever derived from the use of it.

Dr. JEFFRIES.—Mr. Chairman, I did not intend to say any thing on

the question of asthenopia, thinking my report already presented would be sufficient; but, just before leaving home, I received one of the English journals in which the physiology of the ciliary muscle, and also its anatomy, are discussed in such a way as seemed to me very extraordinary. The whole of the experiments of Donders, Helmholtz, Ludwig, and every other eminent physiologist, and their conclusions with reference to the change of shape of the lens as indicated by the images reflected from its anterior and posterior surfaces and from the cornea, are all set aside. The article referred to is in the last number of the Ophthalmic Review, and was written by Dr. Henry Lawson. I was surprised to find such a piece in this journal, which is supposed to hold a high position in the department to which it is devoted. The author has fallen into an error in drawing from analogy (which never proves any thing) after experimenting with the eye of the emu. The idea is, that the ciliary muscles bend the cornea, make it more convex, and elongate the eye. I should infer from that part of the article relating to the anatomy of the muscle, that the one who wrote it entirely neglected to take notice what that muscle is. The circular fibres were discerned by Müller and by Arlt at the same time, without any correspondence between each other, and also by somebody else. Arlt did not assume that the function of accommodation rested with the ciliary muscle, but with the four recti muscles. It was with the idea of sustaining his theory that he went to work very carefully on the eye itself. I saw his way of going to work—the faithfulness with which he investigated every thing. He has given us the anatomy of the muscle very faithfully and very perfectly.

[Dr. Jeffries explained, by the use of a figure on the blackboard, the anatomy of the muscle, and proceeded to criticise the article in question.]

Dr. DYER.—There is a short matter, Mr. President, disconnected from the subject that has been discussed, relating to the facilities for accurately determining astigmatism. I find there is a practical difficulty in using the lines which Snellen has given us, and the difficulty seems to be that the lines are too thick for their length. I divided the square into a greater number of lines, dividing them, however, in the same manner, and making them horizontal or perpendicular, one corresponding to each of the test letters—20, 40, 50, etc. I find them very useful. I have determined it with patients who were suffering from actual astigmatism, and by observations I have had with persons made astigmatic with *cylindrical* glasses. I here exhibit a drawing intended to illustrate it—one made with water colors, which, by the

way, I am not in the habit of using. I intend to have this printed soon.

Before sitting down, however, Mr. President, I wish to relate a series of experiments which I have been carrying on, unsuccessfully, however; but a description may interest some of the members of the society and induce them to pursue this investigation. It occurred to me that if a lens were colored alternately blue and yellow, that with an astigmatic patient the effect of those ought to be green, on the same principle that a wheel painted in alternate sections yellow and green ought, on being revolved, to exhibit green. I find they do not, however; they look more like gray than any thing else. I tried it with a great many patients, both those that were astigmatic by nature and those made so artificially. I have tried various shades, various thickness of the lens, various differences of distance between the lines, tried them with oil colors, water colors, and with crayons. I have pressed several artists into the experiments, but the result has been absolutely and entirely unsatisfactory. Theoretically I can not see how it is.

The committee on the nomination of officers for the ensuing year then reported the following as their nominations :

For President, Dr. Edward Delafield, of New York.

Recording Secretary, Dr. Henry D. Noyes, of New York.

Corresponding Secretary, Dr. Herman Althof, of New York.

Which report was unanimously confirmed.

Adjourned to meet at Boston, Mass., the 2d Tuesday of June, 1866.

PROGRESS OF THE MEDICAL SCIENCES.

I.—HYGIENE.

1. *On the Influence of Social Position on Life.*

It is said proverbially that poverty favors longevity, because the poor are not subjected to the bad consequences of luxury and wealth. Dr. Majer shows the folly of the popular dictum. It has been proved by the researches of Benoiston, De Chateauneuf, De Villermé, Casper, and others, that the value of life is less among the impoverished than the rich. Thus, of an equal number of infants of the same age double the number will die of the poorer than of the wealthier class. Where there is the greatest misery, there is the greatest mortality. According to Casper, the mean duration of life among the better classes of Berlin is fifty years, but among the paupers thirty-two years only. The same writer compares the death-rate of the princely and

noble houses given in the *Almanach de Gotha* with the indigent of Berlin, and he shows that of 1000 infants among the former, 57 die in the first five years; but of the same number among the latter, 345. Whilst the half of the poor only have attained the thirty-second year of life, half of the noble have attained the fifty-second. During epidemics the poorer classes are in an especial manner decimated. That simple well-being prolongs life is demonstrated by the low rate of mortality among persons who "assure" their lives in the assurance offices. A fifth or sixth part of negro slaves die annually. But the mortality among the *free* negroes who serve in the English colored regiments is only 3 per cent., that of slaves being 17 per cent. The learned professions, followed generally by persons of easy means, have an incontestable influence upon the duration of life. Thus, the mean age of fifty-two French literary men was sixty-nine years. Physicians, according to Dr. Escherich, can not hope for a long life. At all periods of age they succumb in larger numbers than other professions. But the greatest mortality is during the early periods. Three-fourths die before fifty years of age, and ten-elevenths before sixty. Old men are rare among them.—(*Annales d'Hygiène Publique et de Médecine Legale*, Jan., 1865; *Canstatt's Jahresber.*, vii., 58, 1864.) (*Half-Yearly Abstract of the Medical Sciences*, etc.)

2. *Researches on Marriages of Consanguinity in the Commune of Batz, near Croisie (Loire Inferieure).*

Dr. Viennois spent a month in Batz, the residents of which have, for centuries, been in the habit of contracting marriages of consanguinity, and of living in nearly total isolation from the neighboring districts. There were 46 such marriages at the time, in the place. He inquired into the previous history of the husband and of the wife; he examined them and their children, with regard to their physical and intellectual development. He questioned the old members of the community also, and with these various materials he drew up tables which show that consanguinity has brought on no disease, no degeneration, no arrest of development, and that the stock has remained very handsome and very pure.—(*Gazette Hebdomadaire de Médecine et de Chirurgie*, Janvier 20 and 27, 1865.) (*Half-Yearly Abstract of the Medical Sciences*, etc.)

3. *Consanguineous Marriages.*

While touching on questions still involved in obscurity, we may briefly advert to the alleged influence of consanguineous marriages on the production of congenital disease or deformity. Several new points are brought forward in the "*Archives de la Médecine Navale*," and in the "*Annales de la Société de Médecine de la Loire*."

The former publication adduces some interesting information on the consanguineous unions of the negro race, and more especially in the posterity of the Portuguese merchant named Da Souza, who died in 1849 at Widah, in the kingdom of Dahomey; all the navigators who have of late years traded on the African coast are well acquainted with these curious particulars. Da Souza, who for many years lived in the country, had acquired a large fortune in the slave trade, and left about a hundred children, the offspring of four hundred women immured within his harem. By the order of the king of Dahomey, hostile to the growth of a half-caste race, this numerous progeny has

been confined in a peculiar inclosure (Salaim) under the authority of one of Da Souza's sons. These wretched mulattoes, unpopular in the country, and strictly watched by the agents of the king, the most absolute tyrant in existence, have no possible intercourse but with each other, or, to speak more plainly, live in the most lamentable state of promiscuity. In 1863, the third generation of children was growing up. The color of their skin is gradually returning to ebony black, although they preserve in their features slight indications of a European origin. Now, it has been ascertained that among all the descendants of Da Souza, who have formed amongst themselves unions between the very closest relations, not a single instance exists of deaf-muteness, blindness, cretinism, or congenital deformity; it is, however, an equally remarkable fact that this herd of human beings is gradually decreasing in numbers, and that its total extinction at no very distant period can even now be predicted.

This decrease does not appear to be referable to consanguinity, and it would be irrational to apply to the posterity of Da Souza an argument not generally admitted in the case of Europeans.

Sterility, as well as polydactylism and other analogous deformities, would seem to be attributable to some other cause, such as, for instance, what Dr. Hervier terms the constitutional predominance of the maternal system. This gentleman recently communicated to the Medical Society of Sainte-Etienne a paper in which this theory is ingeniously defended. The author contends that the social inferiority in which, in all ages and in every nation, women have been kept, is not the result of mere ignorance and prejudice. Every religious cosmogony and every legislation have ratified this universal belief in the dependency and subordination of the female sex, actuated either by an instinct of self-preservation, or by the observation of a circumstance which seems to have escaped the notice of anthropologists since their attention has been directed to the study of the results of consanguineous unions. The fact alluded to is, that in every family in which the constitutional powers of the mother are in a considerable degree superior to those of the father, the offspring is physically or mentally deficient, or affected with congenital deformities, much less frequently observed under opposite circumstances. The author, in illustration of this somewhat hazardous assertion, brings forward several instances of women of Rive-de-Gier, who have given birth to several children by different fathers. "The first children," says he, frequently born out of wedlock, and consequently the offspring of men of superior bodily development, (women uncorrupted by love of admiration, or by want, seldom yielding to any others,) were generally graceful and well-favored; the younger children, on the contrary, were the result of marriage or of illegitimate unions brought about by a craving for luxurious indulgences, or by pressing want, and the fathers were generally inferior in physical development to the mothers. Hence, between the two classes of children, a marked difference in favor of the former, the latter being generally destitute of the physical and moral perfections observable in the others."

These are, in addition, but the results observed from the intercourse of healthy but inharmonious subjects, and "the consequences must be far more striking," observes Mr Hervier, "if one of the parties, or both, labor under some constitutional taint. Under these circumstances, the congenital deviations and deformities referred to consanguinity, such as sterility, polydactylism, deaf-muteness, &c., can scarcely fail to appear."

At Rive-de-Gier, a town containing 15,000 inhabitants, the author has met, in *crossed* couples, in which the blemish alluded to existed, twenty-five instances of sterility, twelve of deaf-muteness, and three of polydactylism; whereas, in upwards of twenty consanguineous but well assorted marriages the children have lived, and may be considered as models of health and symmetry.

Mr. Hervier would not be understood to assert that every case of deaf-muteness, polydactylism, or deformity, hitherto ascribed to consanguinity of the parents, is necessarily referable to the unfavorable circumstances here pointed out, but merely contends that such an origin is admissible; and he therefore suggests a modification of the series of questions now submitted to the parties at the municipal offices before proceeding to the celebration of marriage. He conceives that it would be desirable to ascertain what is the nature of the deformities which the constitutional superiority of the wife over her husband is likely to induce in the offspring; and also, in what proportion differences of age, temperament, &c., between the parents are likely to prove injurious to the preservation and proper development of the human race, at least in a physical and physiological point of view.

The intervention of authorized scientific corporations in the matter does not appear necessary to accomplish the object of the author; it will be sufficient to invite the attention of observers to this curious point of social hygiene, and we have, therefore, deemed the subject deserving of a brief notice in the present article.—*Journal of Practical Medicine and Surgery.*

4. On Preventive Medicine, as illustrated in the proper use of Food.

Mr. Wilson thinks that we are all, especially our children and youths, much under-fed. He recommends three ample meals of mingled animal and vegetable food; and will have “no putting off of the stomach with bread and butter and slop as the effigies of two of the three meals of the day.” Mr. Wilson says:

“But a period comes when milk is no longer the diet of children, and when custom, originating, as we have seen, in Nature’s promptings, has determined the necessity of three meals in the day. The infant demands more than three meals, and makes no distinction between the day and the night. The day of the infant is a day of twenty-four hours; the day of childhood, as of the remainder of life, has a duration of twelve to sixteen hours. The three meals at present under consideration are the morning meal, the mid-day meal, the evening meal. These meals represent the wants of the body arising during the intervening intervals. The morning meal is intended to supply the moderate waste of the night, the mid-day meal the active waste of the morning, the evening meal the active waste of the afternoon. The amount of the three periods of waste is pretty equal; the amount of the supply should be equivalent to that of the waste.

“I am desirous of impressing upon my hearers my opinion and firm conviction that food is not only a necessity, but in civilized life a three-fold necessity, and that the three meals should each represent the third of the nourishment of the day, and be so apportioned as to comprehend an equal amount of variety and an equal amount of nourishment. In the primitive life of the laboring class this law is fully appreciated, and is acted upon to the full extent of their means. With the exception of a somewhat more bulky mid-day meal, the

morning meal and the evening meal do not far diverge from the standard of the mid-day repast.

"But the educated classes are apt to fancy that they possess a knowledge superior to that of Nature, and the result is a perversion of the law of nourishment that leads to the development of debility and disease. A careful, well meaning mother, from purest ignorance—another expression for superior knowledge, the "little" knowledge that is so proverbially dangerous—will tell you that she conforms to the law of Nature in providing for her children three meals in the day. She will describe those meals as breakfast, dinner, and tea, and you will find the composition of those meals to be as follows: A vegetable breakfast, namely, bread and butter, with tea and a little milk; a dinner half animal and half vegetable; and a "tea," vegetable like the breakfast. Here, then, we find education bringing about a total change in the diet of man. Born an animal feeder, he is quickly transformed into a vegetable feeder; that is, more than two-thirds of his diet is vegetable and the remaining third only animal, the exact opposite of that which I consider should be the standard diet of children, namely, one-third vegetable and two-thirds animal.

"My deduction from these premises is, that children are almost universally under-fed, and that the majority of the diseases of children arise from the debility of constitution induced by this habit of under-feeding. If I am right in this view, preventive medicine may do much in the prevention of disease by correcting an error so widely spread.

"The diet of children of all ages should be, a substantial breakfast, with animal food in some shape; a substantial dinner of meat, vegetables, and cereal pudding; and a substantial supper, also consisting, in part, of animal food. The drink may be milk, tea, cocoa, and, possibly, beer. I would call this the diet of health; a diet capable of making a strong body and also a strong mind; and a diet capable of preventing disease. Compare it for an instant with the milk-and-water and bread-and-butter diet of some establishments; the meagre dinner of meat, and the miserable grouting of rice and amylaceous pulp. Rice and amylaceous pulp should have no place in the diet of health, but should be reserved for the sick room.

"Born in prejudice and matured in prejudice, it is the struggle of a lifetime to throw off the trammels of prejudice. We are apt to attach a peculiar signification to the terms which we are in the habit of employing. Ask a person what he usually takes for breakfast, and he will pretty certainly begin his enumeration with the word "tea," the mere drink of the meal; it is, in truth, with him a mere break-fast, instead of being, as it ought to be, a substantial morning meal. The dinner of labor is the luncheon of fashion; then follows the mildly alkaline and stimulating drink that is termed "the tea;" and last of all comes the supper, the late dinner of fashionable life. We have, therefore, before us a succession of three meals and an intermediate drink, but the drink precedes the last meal; and, therefore, the orderly matron, who is more attentive to her 1, 2, 3 than she is to the intention of the daily fare, prescribes for her children breakfast, dinner, and tea—two slops and a meal. But let her, in good English phrase, call the children's meals breakfast, dinner, and supper, and then we immediately obtain two dinners and one slop, the breakfast—an obvious improvement. I have secured to many a child a reasonable evening meal by suggesting to the mother the mere use

of the word "supper" as the name of the third meal. No human being could call bread and butter and tea by the hearty name of supper.

"Assuming that the amount and richness of the supply of food should be determined by the offices which it has to perform, there is no period of life when more food is required than in childhood and youth. The hard-worked laborer in a long summer's day scarcely exhausts a greater quantity of nutritious matter than a growing boy of ten or twelve years of age; in the laborer the consumption is waste; in the growing boy it is bestowed in the construction of the body, in developing and building up the future man. And it is no uncommon thing to find that although the general construction of the body has been fairly performed, there is some one organ of the economy that has fared less well than the rest, and that part not uncommonly the skin; hence the origin of acne, of the ringworms, *et hoc genus omne*.

"If it be admitted that food is the source of the elements of which the body is composed, what kind of body can be expected in the case of a deficient supply of food, whether that deficiency proceed from actual want or from some perverse theory of refinement founded on a false conception of the nature and objects of food, and of its direct convertibility into the flesh and blood of man? Parents are too apt to take their own stomachs as the standard of diet of their children; a cup of tea and a slice of toast suffices for them, so it must suffice for the little ones. I knew a lady who brought up her children on mutton alone, because she herself could digest nothing but mutton. Her children were a feeble, puny, sheepish race, always in the doctor's hands. A mother, in anticipation of the full meal at seven o'clock, can afford a light lunch; but she unfortunately concludes that, because a light mid-day meal is good for her, a spare dinner is equally proper for her children. She has heard somewhere that suppers are heavy and interfere with sleep; so the children must be content with their tea, and go supperless to bed. Parents have rights over their children, but not the right of feeding them in such a manner as to make them the subject of disease. Such parents become the authors of a puny and degenerate race, and are unintentionally traitors to their country.

"If the two periods of life already adverted to be important in their influence on the future man—namely, the period of infancy, ranging from birth to the age of two years, and the period of childhood, ranging from two years to seven years—the next two periods, namely, those of boyhood and youth, are equally so. While the food of the infant and the food of the child are abundant and regular, the food of the boy and the food of the youth should be the same. Both are occupied in the great business of growing life; on both are dependent the future man, for his strength and for his manhood."—(*Medical Times & Gazette*, January 27th, 1865.) *Half-Yearly Abstract of the Medical Sciences, etc.*

5. Wine, mighty Wine.

Dr. Jules Guyot thus says or sings of his country's wines in *L'Union Médicale*:

However far back we look into Pagan, Jewish, or Christian civilization, we find the vine, and its fermented juice, wine, in the highest repute as an inspirer of the human heart.

In Pagan times, the vine and wine had their gods, their priests, and

worship. Wine inspired the poets, and in their mouth celebrated love, glory and genius, the agents of all civilization.

In Jewish days, sacred history everywhere offers us the vine as a symbol of fecundity and riches, and wine as the source of force and contentment of the human heart. From the days of the Deluge the vine and wine are linked to man's existence as forming, in some sense, a part of his first regeneration.

Then comes the Christian era, when we find Jesus showing the necessity of the use of wine in family repasts and social meetings, by the miracle at the marriage at Cana.

The pure fermented juice of the grape—natural wine—is, in fact, the most generous inspirer of the heart of man; it opens a new era to man's spirituality, and conducts him upwards towards perfection, by inspiring him with love and genius. This is one of the great truths inscribed in the sacred writings, accepted originally by ingenuous faith, rejected by pride and philosophic ignorance, but soon recognized by the true science of observation.

Read the history of ancient and of modern peoples, and you may calculate their degree of civilization, their courage, their goodness, their genius, by the use of wine as an alimentary constituent of their ordinary repast.

The surface of the earth is large, but the spots where the vine will grow are small; and we find that true genius has arisen at those spots, and has extended thence, and only into those parts into which wine has been imported.

Where the wine enters not, there exists neither science, nor arts, nor manufactures. Islamism owes its imperfectability and its decay chiefly to its proscription of wine.

There is only one true wine proper to nourish and fortify the heart and the mind of man fraternally and socially and Christianly; and that is the pure juice of the grape, obtained by simple fermentation. Wines that have been boiled, sweetened, and distilled, are no longer natural alimentary and physiological wines. These may please the taste and nourish the body; but they have not the power of uniting men in spirit, and of elevating their hearts.

He who uses grape wine is of good and generous heart, joyous and prompt in spirit; he produces much, and is bountiful to his brothers. He who drinks of other wines is cold and heavy in heart and spirit, and is rather disposed to take than give.

France has for centuries cultivated the grape. First it was established in its hotter provinces; now it is cultivated in eighty departments. To it France owes a great part of its population and its riches, its force, its contentedness, its bravery, its genius, and, above all, its love and devotion to all humanity.

Wines, pure and natural, associated as drinks with ordinary food, are essentially hygienic. Cheap wine is the wine of the people. Drunk in moderation—one bottle to two or three people—it gives force and courage, and nourishes like bread and wine. The wine-grower sells his best wine, and drinks the weakest; but he is strong, and contented, and happy. If he drink beer or cider, he is strong, but he is also saddened. If he have only water, he is feeble and wretched. Tea and coffee have their bodily and spiritual influence; but wine alone bears with it the traditional inspiration of humanity.

Pure natural wines, even when taken to excess at the festive board, do not produce evil results. A free and vivacious gayety, an exaggerated sentiment of universal love and benevolence, are the excessive in-

spirations of good wine. The illustrious Cobden has well said, French wines are the true wines of spiritual gayety; and twenty years hence it will be added, they are the true wines of civilization and universal peace.

The vice of drunkenness is unknown amongst the growers of the vine. The most degraded forms of drunkenness are observed amongst those who partake of falsified wines, and spirits extracted from grains, potatoes, beet-root, etc.

The true qualities of French wines are their purity and primitive simplicity; and they must be appreciated rather by their hygienic and physiological than by their sensual qualities. France, almost alone in the world, produces wines hygienic, physiological, and, as an old Latin inscription over the door of the cellars of the Chateau de Sauvigny has it, theological. These wines offer a thousand shades of difference, corresponding to as many different qualities, and to as many different effects produced upon the mind and body. Every temperament, every social position, can have the wine best suited to it. In the wines of Burgundy we have a generous bouquet, fullness, and warmth, a stimulating and rich taste; they give force and activity to the body, richness of ideas, and gayety spiritual and genial.

The wines of Bordeaux possess a bouquet delicious and unrivaled in expansion, duration, and softness; a taste full, velvety, and balsamic. They render the digestion easy; they give comfort and repose to body and mind, and disturb not the brain.

Champagne wines should figure at all the festivals of families and of nations. They have rendered great services to the spirit of association, and have produced so many happy and fertile ideas, that they may be well considered as powerful elements in the social and spiritual movements of mankind.

The wines of the Alps, the Pyrenees, of the Rhone, the Garonne, the Loire, the Seine, the Moselle, and the Rhine, are treasures of inspiration, of love, bravery, and genius. France is the California of the *esprit* and the heart of men. Its cellars are the richest depots of the concord, the happiness, and the progress of humanity.—*British Medical Journal*, Sept. 9, 1865.

II.—OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN.

6. On Habitual Abortion in Flexion of the Uterus.

It is well known that many women are unable to carry an ovum to the full term of utero-gestation, and that they abort at the same period, in successive pregnancies, without manifest cause. This has been explained by supposing that the first abortion impresses upon the uterus some peculiar weakness or tendency to abort; so that the event recurs, even although the original cause be no longer in operation. The author does not accept this explanation, but believes that every abortion must have some determinate cause, and that the presumed "tendency" to abortion, as a mere result of previous abortions, exists only in imagination.

While, on the one hand, it may very well happen that the so-called habitual abortion, in any patient, may depend upon the continued action of the same cause, it may also depend, each time, upon

a different one. It lies beyond the scope of a journal article to discuss all the possible causes of abortion ; and the author confines his observations to three cases, which appeared to depend upon ante-flexion of the uterus. It is well known that abortion may be produced not only by flexions, but also by other malpositions of the uterus. This has been explained by supposing that the uterus, in its unnatural position, exerts compression upon both its own and the pelvic blood vessels, and thus impedes the return of venous blood from its tissues. In this way is produced a permanent congestion of the uterine parenchyma ; and this, at the period of menstruation, may lead to rupture of the vessels of the hyperæmic uterine placenta, or of the decidua. Such rupture may produce sufficient hemorrhage to detach the ovum and produce its expulsion ; or it may be only enough to threaten an abortion which treatment will prevent.

The ordinary chronic hyperæmia of uterine flexion, which affects the mucous membrane as well as the parenchyma, is much increased at the commencement of pregnancy. Every flexion also modifies the changes of position of the gravid uterus, which, through its increasing weight, becomes more or less ante- or retro-verted. Hence there is an addition to the ordinary impediment to the return of venous blood, and, at the same time, an unusually active flow of arterial blood. The increase of the ante- or retro-version is what chiefly requires attention ; since this, and not the flexion, is the exciting cause of abortion.

Anteflexion is the malposition that most frequently causes habitual abortion, although it may often not be recognized at the time, being much diminished by the contraction of the longitudinal muscular bands, so that the uterus may come to have its long axis coinciding with that of the pelvic entrance. As involution takes place, the former flexion reappears.

As practical rules, the author lays down the following : 1. If a woman suffering from flexion become pregnant, she should be examined in the second, or, at latest, in the third month of her pregnancy, in order to ascertain what changes of position the uterus has undergone. 2. In any case in which abortion has already commenced, without assignable cause, an examination should be made some time afterwards, in order to ascertain whether there be any flexion present, which, during the actual period of abortion, was for the time overcome by muscular action.

In mentioning the want of statistics upon the question of the connection between flexion and abortion, the author points out that the power to produce an injurious degree of congestion must depend upon the degree of the malposition ; and that the slighter grades may be entirely harmless.

The rational treatment consists in an endeavor to remedy the flexion in the non-pregnant state, so as both to facilitate conception, and to prevent the recurrence of the dangerous ante- or retro-version when pregnancy takes place. The absolute cure of a flexion can only very seldom be effected ; but sufficient improvement for the end in view may generally be obtained. After such improvement, it is still necessary to examine the position of the uterus in early pregnancy, and to see if it can be modified with advantage.

The author's treatment consists in confining the patient to bed eight days before the time at which the second menstrual period would occur if she were not pregnant ; and in a daily endeavor to rectify, by the fingers, any malposition that examination may detect. If the tendency be towards anteversion, the patient must lie on her

back; if towards retroversion, on her side. She must remain in bed for eight days after the menstrual time, continuing the same daily treatment, and then may rise, and move about cautiously, avoiding strong exertion. Eight days before the third period she must return to bed, and again undergo daily manipulation; but, when the first week of the fourth month has passed, if the uterus (as is usual) has then risen well above the pelvic brim, all fear of ante- or retro-version may be laid aside, the patient may leave her bed, and return to her ordinary mode of life.

The author concludes by expressing his belief that flexions of the womb are not cured by ordinary pregnancy, but that they are reproduced after labor at full time.—(*Monatsschrift f. Geburtsk.*, September, 1864; and *Schmidt's Jahrbücher*, No. 2, 1865.) *Half-Yearly Abstract of the Medical Sciences, etc.*

7. *Amenorrhœa.*

The history of ovulation has supplied M. Raciborski with a new field of inquiry, which he has laboriously cultivated, and in which he has succeeded in discovering new physiological aspects unknown to, or at least not described by, his predecessors. Amongst other interesting subjects, he expatiates on a form of amenorrhœa due to mental causes, such as excessive dread of pregnancy, or, on the contrary, an inordinate desire of bearing children. (*Archives de Médecine*, May, 1865.)

In the male mental pre-occupation greatly influences the procreative function. Montaigne, in his remarks on the power of imagination, relates an instance of transient sexual incompetency, of which, in all probability, he had himself been the subject. Incapacity of this kind is of frequent occurrence, and inspires no anger to a sensible wife, aware that kindness will prove far more successful in restoring power than bitter reproach and offensive expressions of scorn. The physiological explanation of this unsatisfactory condition is simple. In consequence of the apprehension of failure, the mental frigidity is conveyed by the sympathetic system of nerves from the brain to the organs of generation, and the result is an entire cessation of their powers of expansion. Under the influence of the vaso-motor nerves, the blood vessels of these organs contract, the temperature of the part is lowered, and a condition ensues in which sexual access becomes impracticable.

M. Raciborski, arguing from analysis, opines that the excessive dread of pregnancy, or the immoderate desire of bearing children, act on the female in a somewhat similar manner, and may induce more or less delay in the appearance of the catamenia, and even a protracted state of amenorrhœa.

This author was consulted on several occasions by women who, after a long struggle, had yielded to their feelings and forgotten their virtuous resolutions. Alarmed at the possible consequences of their imprudence, and living in perpetual terror of pregnancy, they impatiently counted the days which must intervene before the return of the menses, and anxiously watching for the usual premonitory symptoms, awaited in a state of most distressing perplexity the time at which their worst fears might be allayed or confirmed. In a case of this kind, a lady, usually perfectly regular, was thrown by a delay of one week into a state bordering on insanity. The treatment adopted by M. Raciborski consisted in arguments calculated to remove her fears, to which he mainly attributed the alarming postponement of the

catamenia, and in the exhibition of harmless remedies. He prescribed gentle anodynes, and the mildest form of stimulants, such as a few drops of liq. ammon. acetatis in lime-flower, or black-currant tea, and mustard foot-baths. After an interval of two days, the menses reappeared, and the delighted patient solemnly declared that the lesson would never be forgotten.

On the other hand, M. Raciborski asserts that too ardent a wish for children may also act in a reflex manner on the vasso-motor nerves of the ovaries, and induces amenorrhœa.

"In young married women," says he, "it is not unusual to observe at several successive monthly periods a delay of a few days before at last they become really pregnant. These delays are, in a certain degree, referable to a strong desire to have a family. When, however, several months have elapsed without any sign of this much wished for result, the anxiety on the subject often becomes excessive; and constantly preoccupied with one idea, that she may be sterile, the young wife feels happy when, at the return of the date at which the menses may be expected, she experiences none of her customary symptoms; she hopes that the catamenia may fail, and that at last she is pregnant. At each monthly period she is agitated by the same hopes, and, to avoid a disappointment, submits to all manner of precautions. Between this kind of amenorrhœa and that we have previously described, a considerable difference exists. In the former, when the patient dreads pregnancy, every effort is made by her to bring on menstruation, which, in general, reappears in the course of a few days. In the latter, on the contrary, all the precautions taken to prevent the frustration of cherished hopes, the absolute repose joyfully submitted to, the complete abstinence from any cause of mental or physical excitement, all contribute to perpetuate the modified condition of the ovarian circulation, and to protract the duration of the amenorrhœa. The greater number of the cases of what has been termed *Grosesses nerveuses*, recorded by various authors, have no other origin, and are almost invariably instances of protracted amenorrhœa referable to this cause."

M. Raciborski relates an interesting case in point; but his remarks on the variety of amenorrhœa *due to the apprehension of pregnancy* appear to us original, and deserving of the attention of the practitioner.—*Medical Circular*, August 2, 1865.

8. *On the influence of Uterine Displacements upon the Sterile Condition.*

Dr. Marion Sims said that we were all interested in the subject of sterility, when we remember the fact that every eighth marriage was sterile. He did not propose then to give us a complete paper on the subject, but only to present it in one of its relations, viz.: that of its dependence upon misplacements of the uterus. He divided his sterile patients into two classes: 1st. Those who were married a sufficient length of time and did not conceive; 2d. Those who had borne children, but for some reason ceased to do so long before the termination of the child-bearing period. The first he called "natural sterility;" the second, "acquired sterility."

To show the frequency of uterine displacements in this relation, he said that of 250 cases of "natural sterility" that had fallen under his observation, 103 had anteversion, and 68 retroversion; and of 255 cases of "acquired sterility," 61 had anteversion, and 111 retroversion,

the anteversions predominating in the first class, the retroversions in the second—the two opposite displacements being almost in inverse proportion in the two classes, and forming about two-thirds of the whole number, being 343 out of 505 cases; which proved beyond question the bearing and importance of these displacements in connection with the sterile condition. He then illustrated by diagrams the normal position and relations of the uterus, explained the various causes and complications of anteversion, whether dependent upon fibroid tumors, elongation of the infra- or supra-vaginal cervix, shortening of the utero-sacral ligaments, or hypertrophy of the fundus. In all these cases, he said, we could not do much for the relief of the sterile condition by merely mechanical means; that our efforts should be directed to seeing that the os tincæ was properly open, that the canal of the cervix was free from engorgement, and that the secretions, both vaginal and cervical, were not poisonous to the spermatozoa. He said that there was one form of anteversion that was easily cured by a simple and novel operation, which he originated some eight or nine years ago. He illustrated this by cases and diagrams. It was as follows: The uterus lies down on the anterior wall of the vagina, and parallel with it. The fundus is most usually the seat of a fibroid growing anteriorly. The anterior wall of the vagina is greatly elongated, the os tincæ pointing directly backwards. Under these circumstances, he has shortened the anterior wall of the vagina an inch and a half, by denuding a surface a half inch wide and two inches long across the axis of the vagina in juxtaposition with the cervix uteri, and making a similar transverse scarification parallel with the first, about an inch and a half, more or less, anteriorly to it, and then uniting these two transverse cut surfaces by silver sutures, just as we would unite the edges of a transverse vesico-vaginal fistula by them. This necessarily shortens the elongated anterior wall of the vagina, draws the cervix forwards into its normal relations, and as a consequence elevates the fundus. He related several successful cases of this operation, and had seen it followed by conception and child-bearing. He then passed to the consideration of retroversion as influencing the sterile condition, pointed out its varieties and anomalies, and showed how it was to be diagnosed and how replaced. By diagrams he illustrated various modes of reduction, showed how conception was difficult, and sometimes impossible, in some forms of retroversion, advocated mechanical treatment, pointed out the dangers of pessaries, but advocated their use when judiciously applied under proper circumstances. He prefers a malleable ring, either of block tin or a ring of copper wire covered with gutta percha, and then bent or curved to the proper diameters of the vagina of each patient. He said this was a modification of Hodge's pessary. Under some circumstances he also uses Meigs' ring pessary, made of watch spring covered with gutta percha. He pointed out the peculiar advantage of each of these, and paid a just tribute to his countrymen, Drs. Hodge and Meigs, who were the earliest advocates of mechanical treatment of uterine displacements. He said that the great secret of treating the sterile condition when dependent upon retroversion was to adjust a malleable ring which would hold the uterus in its normal position, and which was to be worn always during the act of coition. He explained its philosophy, its efficiency, its safety, and its harmlessness, and related a great many cases in which its use had been followed by conception; one after a sterile marriage of six years, another of ten years, another of fifteen years, and others at various periods of time after

sterile marriages. He also showed how miscarriages, often dependent upon this displacement, are prevented by the use of a properly fitted malleable pessary. He then pointed out the course to be adopted when it was impossible for the patient to wear a pessary, showing why it was so, and what was to be done.—*Medical Times & Gazette*, August 19, 1865.

9. *Case of Puerperal Tetanus, following Abortion and Plugging of the Vagina.* By JOSEPH BLACKSHAW, Esq., Stockport.

On Saturday, November 8th, 1864, I was called to see Mrs. H., aged 48 years, the mother of a numerous family, of a highly nervous temperament, and whose general health had previously suffered from some domestic anxieties.

She was in bed, very faint from profuse hemorrhage from the uterus. I made an examination, and detected an ovum of about ten weeks' growth within the os uteri. In consequence of the amount of the hemorrhage, I plugged the vagina; ordered cold applications, stimulants, and the usual astringent remedies, including the ergot of rye; and she rallied in the course of the following day. The plug remained in the vagina twelve or fifteen hours, and, when removed, was not again resorted to, as the hemorrhage had almost ceased, and the ovum was expelled a few hours afterwards. She progressed satisfactorily for about nine days, at the end of which time she was able to sit up, and about to leave her room. Thinking it unnecessary for me to continue my visits daily, I left her, with directions to report to me her progress.

On the day following, (Tuesday,) just ten days from my first visit, I was unexpectedly requested to see her. She thought she had taken cold, and was then complaining of great stiffness of the deep-seated muscles of the neck and throat, with difficulty of swallowing, and was unable to open her mouth perfectly. As there was some febrile excitement, she was ordered to remain in bed, and to take a saline mixture with an aperient; to use hot fomentations; afterwards hot moist bran; to steam the fauces; and, if possible, to use a gargle.

On the two following days, Wednesday and Thursday, the painful tension of the masseters, as well as the muscles of the neck and throat, had so greatly increased as to prevent deglutition and bring on a state of perfect trismus. On Thursday afternoon and evening, the tetanic seizures became increasingly frequent, producing great muscular rigidity, contorsion of features, and slight opisthotonos. During the paroxysm, the pulse was small and feeble; but the consciousness was entire throughout. She continued in this state until Saturday evening, the tetanic spasms and opisthotonos gradually becoming more severe, when she died from exhaustion, five days from the first setting in of the tetanic symptoms.

Owing to the clenched state of the jaws, little was done in the way of treatment, beyond a little counter-irritation to the spine; the administering of a turpentine enema; and the removal of faecal accumulations, which might prove a possible source of spinal irritation.

Puerperal tetanus is a very rare species of that disease in this climate; and this must be my apology for this communication. I have been in practice in this town for more than thirty years, the greater part of which I have been medical officer to a large district of a poor-law union, including the union workhouse, and must have attended

three or four thousands of women in labor at different periods of uterogestation; and this is the first case that has occurred in my public or private practice, or, so far as I can learn, in that of the oldest obstetrician either here or in Manchester, with the exception of one case mentioned by Dr. Whitehead. Dr. Radford considers it a rare disease, but has known it to occur. Mr. Robertson, of the same city, also says that obstetric tetanus is a novelty to him, and that he has never seen it during a long and laborious practice. Nor is the disease one usually recognized in treatises on midwifery and the diseases of lying-in women. The extreme rarity and infrequency of this disease, notwithstanding the various lesions from the application of instruments, manual interference, turning, and other violence that the uterus must often sustain in protracted and difficult labors, points to a very different state of the nervous system from that which gives rise to traumatic tetanus in the various external injuries to which the body is subjected. Physiologists attribute this to the uterus receiving its nerves from the great sympathetic. It may be so. But of what that peculiarity of the nervous system consists in these cases, where the irritation from the uterus, as in the case of Mrs. H., is propagated to the cerebro-spinal system, is still shrouded in mystery. We can only say that, in the case of my patient, cold applied to the body, previously lowered by mental anxiety and loss of blood, may have given rise to a state of reflex spinal irritation, followed by tetanus; though probably, in ten thousand other instances, the same exciting causes would produce no such effects.

I regret that chloroform was not tried, as, besides the good effects it is said to have in convulsive diseases, it presents facilities of application where no internal remedy can be given.

My object, in this communication, is rather to seek than to give information, and I trust that some of our more learned associates will ere long throw some fresh light upon this class of diseases, which are yet among the *opprobria medicorum*.—*British Medical Journal*, Sept. 9, 1865.

10. *Electro-magnetism in Post-partum Hemorrhage.*

In a communication to the editor of the *Medical Times & Gazette*, Mr. Parsons, of Liverpool, relates the following interesting particulars:

The following case, illustrating the beneficial effect of electro-magnetism in post-partum hemorrhage, is, I think, worthy of record, for it more than answered my most sanguine expectations, after hand pressure on the uterus, pressure on the abdominal aorta, ergot, and the cold douche had failed. Mrs. B., a fine healthy young woman, twenty-seven years of age, was suddenly seized with violent labor-pains, at 1 o'clock p.m., November 24, 1864. She sent immediately for me, but I was unfortunately out at the time. Messengers went in every direction for a medical man, and a considerable time elapsed before any assistance could be procured. Dr. Watters, St. Anne Street, at length arrived, and found one child born and Mrs. B. flooding a good deal. He detected another child in the uterus, made pressure on the fundus, and, presentation being natural, delivery was easily and rapidly effected. The flooding now became profuse and alarming, and Dr. W., finding the placenta adherent, introduced his hand into the uterus, cleared out its contents, applied a bandage and pad and

cold to the vulva. My assistant, Mr. Burrows, now arrived, and Dr. W. left the case in his hands. Notwithstanding all his efforts to arrest the hemorrhage, Mr. Burrows saw that the patient was sinking rapidly. He tried ergot without producing any uterine contraction. He then removed the binder and grasped the uterus, directing the attendants to administer brandy freely. He tried the cold douche; but still the hemorrhage continued. He then made pressure upon the abdominal aorta, and for the first time he observed a beneficial result; the flooding was arrested. He kept up the pressure for an hour. I then arrived, (at half-past three o'clock,) and was shocked at the sight which met my gaze on entering the room. The patient, anæmic, cold, almost pulseless, was lying in a pool of blood. There was only a slight draining from the uterine sinuses. I felt the uterus through the abdominal walls: it was large, flabby, and felt empty. I passed one hand into the cavity and removed a few small clots, at the same time keeping up pressure with the other hand. Finding that neither pressure nor the irritation of the hand in the uterus excited any muscular contraction, it occurred to me that electro-magnetism was our *dernier ressort*. I relieved Mr. Burrows in keeping up the pressure on the abdominal aorta, and directed him to drive to my house for my electro-magnetic apparatus. He arrived in a few minutes, and I proceeded without delay to apply one pole inside the uterus, while the other was being applied externally over the abdominal walls. The lowest power was first used, and then gradually increased to a medium. In a few minutes I felt slight contraction, and after continuing the current for half an hour I was enabled to grasp the whole of the uterus in my hand. I retained it in this manner for better than half an hour, and then applied a tight binder and pad. After clearing away as much of the *debris* as possible, Mrs. B. became quite conscious, and complained of being cold. I gave some brandy and hot water, and, fearing to continue hot drinks, I put extra clothing over her, and directed a female to lie beside her. When she became warm she fell into a doze. I left her for a short time now, 5 o'clock.

At 6 o'clock very sick; skin warm; pulse 140; complained of binder being very tight. Ordered chloric ether and spt. ammon. co. every half hour; the binder to remain. No flooding.

Half-past 9.—Improving in every respect. Ordered the mixture every hour; cold beef-water for drink.

Mrs. B. continued to improve daily, and was afterwards enabled to nurse both children. She is now, August 16, 1865, as florid and healthy looking as ever.

In this case the patient owed her life, in the first place, to the pressure which was maintained so persistently on the abdominal aorta, and most assuredly also to electro-magnetism. Had transfusion been used here the vital fluid would have permeated the system only to find an exit through the patent uterine sinuses. I have used the electro-magnetic fluid in one other similar case with the like beneficial result. *Medical Times & Gazette*, Aug. 26, 1865.

11. Retention of Urine in the Fœtus.

M. Depaul related to the Société de Biologie a case in which a woman was delivered of an eight months' child presenting a great enlargement of the abdomen; there was very little amniotic fluid. The child died soon after being born. The bladder was about $4\frac{1}{2}$ inches long and $2\frac{3}{4}$ wide, and was full of urine. The ureters were also

irregularly enlarged, resembling, at first sight, the intestinal convolutions, and contained urine. The kidneys, especially the left, were also much distended; they were transformed into cysts with thin walls, filled also with urine. The cause of this distension was found to be an imperfect state of the urethra, at the junction of the muscular and prostatic portions. More than 500 grammes (upwards of a pint) of urine were removed.—*Gaz. Méd. de Paris*, July 15, 1865.

12. *Relief in Cancer.*

Dr. Brandini, of Florence, has recently discovered that citric acid will assuage the violent pain which is the usual concomitant of cancer. One of his patients, aged 71, at the Hospital of Santa Maria della Scala, was afflicted with cancer on the tongue. There was no possibility of performing an operation, the surface attacked being far too extensive, investing the base, the sub-lingual, and the sub-maxillary glands. The poor man in the midst of his torments asked for a lemon, which was nothing very remarkable, as cancerous patients generally have an extraordinary liking for acids. But the seat of the disorder being in the mouth, a circumstance was observed which might otherwise have escaped attention—the juice of the lemon diminished the pain.—*Medical Times & Gazette*, August 26, 1865.

EDITORIAL.

THE CHOLERA STILL HOLDS ON.—The hesitancy in its march, when first it appeared in Europe, caused those who chronicled its progress to entertain the hope that it would retire to its Asian home and leave broad Europe free. Recent accounts have, however, dissipated these hopes—for the hesitation was apparent, not real. In Marseilles there have been a sufficient number of deaths to awaken great alarm, to cause many of the families to flee the city, and to produce great consternation among those remaining. In Malta and Gibraltar, in Ancona, and many towns along the Italian coast, it still prevails with unrelenting violence. We have no official reports that it has appeared as an epidemic in Paris or London, though in the latter city cases are reported in the weekly tables of mortality.

Thus far, then, no faith is to be put in the statements that this visitation of the epidemic will expend itself in its original haunts; nor can we escape a fearful responsibility if, with these warnings, and this favorable period of the year, we fail to set our house in order, to prevent, as far as possible, its advent during the next year.

—The Academy of Medicine opened its doors after the summer vacation on Wednesday, the 20th of September. Dr. Elisha Harris read a paper entitled “Hygienic Observations in New Orleans,” illustrat-

ing the utility of efficient sanitary regulations ; and Dr. John H. Griscom presented an historical account of the epidemic cholera, showing how its progress can be checked by proper and prompt attentions to the laws of hygiene.

— This month the regular courses of lectures in the various medical colleges of this country will begin. By referring to our advertising pages it will be seen that in some of these institutions the effort to offer a thorough course of instruction can not be other than successful, if the number and character of the instructors be a criterion of success.

The Regular Term in the Bellevue Hospital Medical College commences Wednesday, Oct. 11. We understand that the vacancy in the Faculty, occasioned by the sudden death of Prof. T. Childs, has been filled by the temporary transfer of Prof. Stephen Smith from the Chair of Surgery to that of Anatomy, and that the duties heretofore devolving upon Dr. Smith as Professor of the Principles of Surgery will be added to those of the Chair of Military Surgery, filled by Prof. Hamilton.

The regular course of lectures in the College of Physicians and Surgeons will commence on Monday, Oct. 16.

In the Miami Medical College of Cincinnati, and the Medical College of Ohio, the regular lectures will commence on the 1st of November.

— The operation for Ovariectomy has recently been performed in Italy for the second time. Prof. Bezzè, of Modena, has operated with success, and this happy issue of a formidable operation, which has been heretofore ignored in Italy, will probably be the cause of many other operations in that country.

It would seem that on the continent of Europe the success of this operation, under the hands of the local surgeons, has been any thing but satisfactory. And it would appear that English surgeons, who, at home, have a great reputation for this special operation, are not any the more fortunate when operating on the continent—for Mr. Spencer Wells, who enjoys high distinction in England, failed in an operation he performed in Brussels, by invitation of M. Deroubaix, the patient dying the fourth day from peritonitis.

Dr. Dutoit, of Wurzburg, says *L'Union Médicale*, has collected all the cases of this operation up to Nov., 1863, making, in all, 742, of which 467 were in England, 165 in America, 74 in Germany, 28 in France, and 8 in other countries. Dr. Dutoit attributes the successes of the English surgeons to the fact that they enjoy the entire con-

fidence of their patients—to such a degree that they have but to propose an operation, however severe it may be, to readily gain the assent of the patient; while on the other hand, the continental physician has to exhaust all his resources of argument to convince his patient of the necessity of the operation, and as the danger to life is not immediate delays are always urged, until complications arise which diminish greatly the chances of success.

At a meeting of the Physicians and Surgeons at the Demilt Dispensary, held August 23d, Dr. D. S. Conant in the chair, on the occasion of the death of Dr. Thomas Sinclair, the following preamble and Resolutions were reported and unanimously adopted:

Whereas, In God's providence, our fellow-physician, Thomas Sinclair, M.D., L.L.D., has been removed from us by death; therefore,

Resolved, That in the death of Dr. Sinclair, the Demilt Dispensary, in which he has so earnestly and so disinterestedly labored, is called to mourn one who, by his devoted and intelligent efforts, largely contributed to realize the benevolent objects of the institution; and that we have to individually lament a friend, who, by his mature scholarship, unflinching professional zeal, uniformly courteous and unobtrusive manners, simplicity of character and kindness of heart, had won, in a high degree, our esteem and respect.

Resolved, That we tender to the family of the deceased our sympathy for their bereavement, and that we attend the funeral in a body.

Resolved, That a copy of these resolutions be transmitted, by the Secretary, to the bereaved family, and copies for publication to the *New York Times* and to the *New York Medical Journal*.

ISAAC CUMMINGS, M.D.

WM. B. BIBBINS, M.D.

JAS. R. LEARNING, M.D.

Committee.

HENRY M. FIELD, M.D., *Secretary.*

LONGEVITY IN ENGLAND AND WALES.—The returns of mortality for England in the year 1863 record the death of 213 men and 430 women registered as 95 years old or upwards when they died. Twenty-one of these had reached 100 or upwards, and one at Chelsea was 109. Sixty-two of the women had also completed a century of life or more; and one at Liverpool, in the district of West Derby, was 112 years old. Five men and five women died in the year 1863, who, if the register may be relied on, were born before George III. was king—that is, before 1761. It appears that longevity is lowest in the North Midland division of England, and by far highest in Wales, that which approaches nearest to the Welsh being the South Midland division of England. The proportion of those who reach the age of 100 years in Wales as compared with the North Midland division of England is about 10 to 1 in favor of the former, while Wales has the advantage of more than 2 to 1 above England's most favored district.

BOOKS AND JOURNALS RECEIVED.

The Medical Register of the City of New York, for the year commencing June 1, 1865. Published under the supervision of the New York Medico-Historical Society. Guido Furman, Editor. New York: Edward O. Jenkins. 1865.

The Use of the Laryngoscope in Diseases of the Throat, with an Appendix on Rhinoscopy. By Morell Mackenzie, M.D., Lond., M.R.C.P., &c. Philadelphia: Lindsay & Blakiston. 1865.

The Physician's Visiting List, Diary, and Book of Engagements, for 1866. Philadelphia: Lindsay & Blakiston.

On the ultimate Nerve Fibres distributed to Muscle and some other Tissues, with observations upon the Structure and probable mode of action of a Nervous Mechanism. Being the Croonian Lecture for 1865, delivered by Lionel S. Beale, M.B., F.R.S.

Canada Medical Journal. Vol. 2, Nos. 1, 2, 3.

The British and Foreign Medico-Chirurgical Review. No. 71. July, 1865.

British Medical Journal. August 19, 26; September 2, 9, 16.

The Lancet. August 19, 26; September 2, 9, 16.

The Medical Circular. August 23, 30; September 6, 13.

Medical Times and Gazette. August 19, 26; September 2, 9, 16.

The Medical Press, Dublin. August 9, 16, 23, 30; September 6, 13.

Archives of Medicine. Nos. 14, 15.

American Journal of the Medical Sciences. July, 1865.

American Druggists' Circular. July, August.

American Journal of Pharmacy. July, 1865.

Boston Medical and Surgical Journal. June 22; July 6, 13; August 3, 10, 31; September 14, 21.

Buffalo Medical and Surgical Journal. July, August.

Chicago Medical Examiner. July, August, September.

Chicago Medical Journal. July, August, September.

Cincinnati Lancet and Observer. July, August, September.

Dental Cosmos. July.

Nordamerikanische Deutsch Medicinische Zeitschrift für Praktische Heilkunde. Buffalo. Aug.

Pacific Medical and Surgical Journal. June, July, August.

The Medical News and Library. July, August, September.

Braithwaite's Retrospect. Part 51. July, 1865. New York: W. A. Townsend.

American Literary Gazette & Publishers' Circular. Philadelphia: July 1, 15; August 1, 15; Sept. 1, 15.

Addresses at the Fifth Annual Session of the American Dental Association. By W. W. Allport, D.D.S., Daniel Brainard, M.D., and N. L. Davis, M.D., Chicago. 1865.

The Catholic World. July, August, September, October.

Annual Circular Bellevue Hospital Medical College. New York: 1865-66.

Sixth Annual Announcement of the Miami Medical College of Cincinnati.

Twenty-third Annual Announcement of Rush Medical College, Chicago, Ill.

Annual Circular of the National Medical College, Washington, for the Session of 1865-66.

NEW YORK MEDICAL JOURNAL,

A MONTHLY RECORD OF MEDICINE AND THE COLLATERAL SCIENCES.

NOVEMBER, 1865.

ORIGINAL COMMUNICATIONS.

Contributions to Obstetric Jurisprudence. By HORATIO R. STORER, M.D., of Boston, Assistant in Obstetrics and Medical Jurisprudence in Harvard University; Surgeon to the New England Hospital for Women; and Professor of Obstetrics and the Diseases of Women in Berkshire Medical College.

NO. IX. A MEDICO-LEGAL STUDY OF RAPE.

Following the example of my friend, Dr. Elliot, who has transferred to this Journal, from the now silent pages of the *American Medical Times*, the continuation, in regular sequence, of his series of Difficult Obstetric Cases, I recommence my own "Contributions to Obstetric Jurisprudence," which were initiated in 1859 in the late *North American Medico-Chirurgical Review*, of Philadelphia, and interrupted by the cessation of that journal. The articles already published under the above title are the following:

I. Is Abortion ever a Crime? *N. A. Med.-Chir. Rev.*, Jan., 1859, p. 64.

II. Its Frequency and the Causes thereof. *Ibid.*, March, 1859, p. 260.

VOL. II.—No. 8.

III. Its Victims. *Ibid.*, May, 1859, p. 446.

IV. Its Proofs. *Ibid.*, p. 455.

V. Its Perpetrators. *Ibid.*, p. 465.

VI. Its Innocent Abettors. *Ibid.*, July, 1859, p. 643.

VII. Its Obstacles to Conviction. *Ibid.*, Sept., 1859, p. 833.

VIII. Can it be at all controlled by Law? *Ibid.*, Nov., 1859, p. 1033.

To this series of papers there already belong, in reality, six others, bearing as they do directly upon the medico-legal relations and responsibilities of women: to wit, three articles upon Insanity in Women, published in the *Boston Medical and Surgical Journal* for April, October and November, 1864; a fourth upon the same subject, that will be found in the Transactions of the American Medical Association for that year; still another, read before the Association at its late session in Boston, as the Report of the Standing Committee upon Insanity; and a sixth, my Prize Essay, upon the Physical Evils of Forced Abortions; both of these latter articles being now in press. I have, however, preferred to take the thread up where it was broken, in pursuance of my original plan, with the simple prefatory remark that in no department of Obstetrics and the Diseases of Women, or of Legal Medicine, is there greater need of investigation, and in none more prospect of practical gain to our science, than where these fields join each other, namely, in Obstetric Jurisprudence.

The subject upon which I desire at this time to fix the attention of medical jurists, is a more practical and important matter than might at first be supposed. It is one concerning which much conflict of opinion has arisen, both upon abstract points and the circumstances of individual cases; it is also one of those happily rare questions where the sympathies of a jury are apt to be found instinctively tending against the prisoner.

It may be supposed that a topic upon which so much has been written, and which possesses an interest, even if unconfessed, for every professional reader, must long ere this have been exhausted. In reality, however, it will be found, that just in proportion as it has received attention, so have the medico-legal opinions concerning it, prevalent at any given time, been modified. The crime was formerly punished by

castration or by death ; its penalty now varies in different countries, but is almost everywhere confined to fine and imprisonment, either or both. For its commission it was formerly necessary that every step and stage in sexual intercourse should have been completed; now the mere fact of contact of the genital organs would seem practically to constitute the offense. So that the literature and the law of rape have alike become effete and need careful revision.

I would not detract from the labors of the late Casper of Berlin, and his many predecessors in this discussion, and freely acknowledge the excellence in many respects of the rulings and reasonings of Wharton and Stillé, our own latest authorities; but in certain material points I am compelled to differ from them all.

A case that has lately occurred at Boston, where four young men have been sentenced *to the State-prison for life* for compelling intercourse from a notorious strumpet, and their so severe punishment, apparently indorsed by the entire community, warrants my examining into details that, though repulsive in themselves, are of value as establishing upon a firmer basis the right of a woman to her chastity, however infinitesimally small this may be. The right involves that of distributing her favors, such as they are, or withholding them altogether; the right to refuse to obnoxious purchasers wares that may have been exposed in open market; and the right to cancel or continue consent to an act, however unlawful in itself.

The instance to which I have now referred, that known as the Bates case, has created such intense excitement throughout New England, and is withal so directly pertinent to conclusions I may hereafter enunciate, that I have thought best here, at the outset, to present a brief commentary upon its more salient points. This has been kindly furnished me by the only medical witness called for either side at the trial referred to, my friend Dr. John Green, of Boston, one of the Surgeons to the City Dispensary, to whose interesting experiments and intelligent deductions concerning certain matters connected with the case I may take occasion subsequently to allude. The case, as I have said, was that of a courtesan, abused by several men; the conviction turning upon the testimony of one of their

comrades, incapacitated by acute disease from sharing in the fray. The criticisms now offered I believe to be strictly just.

"The chief points, as they strike me," says Dr. Green, "are:

"1st, The utterly bad character of the prosecutrix and her bad appearance on the stand, lying, as she did, upon many points which could be proved against her by good witnesses, but being supported by the sick soldier as to the particular points necessary technically to constitute a rape;

"2dly, The proof, by an eye-witness, of those essential points which are usually settled by presumption, viz., the unwillingness of the woman, her resistance, and the use of force by the accused;*

"3dly, The insignificant injuries, if any, which were received by the woman, and which, in the absence of other positive testimony, would have weighed most heavily against her statement that she resisted.

"As regards the character of the woman, there can be no doubt; married to a man named Pettingill, she has been since publicly known as the concubine of Mellen Bent, and now of the witness Bates, whose names she has successively borne. She has been proved in the habit of walking the streets at improper hours of the evening, and in neighborhoods and upon routes which leave no doubt that she was also a most degraded common prostitute. Her visit to the room where the offense was committed was against her will, as she said, but entirely voluntary, according to another witness, whose appearance was at least much better than hers. Her description of the acts of violence was contradictory in important points, so much so that it would have been easy completely to have demolished her testimony, had it not been confirmed in the essential points by the sick soldier, who was an eye-witness to the circumstances attending the intercourse.

"The facts proved by this witness were:

"1st, Much rough usage, constituting an assault; and

"2dly, Connection by the four defendants in turn, and once or more repeated; and

* On the other hand, it ought to be the presumption, in any case, on the ground of the prisoner's assumed innocence, that the woman was not unwilling, but consented.

"3dly, That the connection was effected by the use of superior force, to which the woman wisely yielded without very violent resistance—having no honor to defend, and consequently no very strong instinct to save herself from embraces to which she would willingly have sold herself, and had, perhaps, previously bargained for with one of the accused.

"That the accused had probably no idea of the terrible nature of the offense of which they have since been thus technically convicted, is shown by the fact that they retired to rest in the very room in which it was committed, and where they were afterwards arrested. They were rough boys, and had dealt roughly with a whore—a crime, it is true, but certainly one not to be compared with the brutal violation of a virgin or chaste matron.

"As regards the line of defense, there was nothing to be done. Enough was proved to show that *technically* a rape had been committed, and the court imposed the only sentence permitted by the statute. In *reality*, the case lacked every feature of horror usually belonging to the crime, and seems to call for the exercise, at a very early period, of the pardoning power vested in the Executive.

"The details of the evidence have been pretty fully reported in some of the papers; and will, I think, fully sustain the views which I have expressed; certainly they do not justify the congratulations of the daily press over the speedy bringing to justice of the perpetrators of an almost unprecedented outrage."

It is not, however, the merits or demerits of merely isolated cases, however interesting, that I would at this time discuss. The war, now so happily over, has suddenly loosed from the strict bonds of enforced discipline hundreds of thousands of able-bodied men whose blood has been heated by a Southern sun and by long privations, now replaced by the license of victory, of return to their homes, and of freedom from the hard but necessary thralldom of the camp and field.

At no time in the history of this country have the self-restraint and moral sense of the masses been so severely tested as at the present; at no time, if we may believe current reports, have so many forcible attempts been made upon the chastity of women. Allowing that flying rumor may have

magnified every instance that has really occurred into a hundred, still no one can deny that there has been abundant cause for general anxiety, if not alarm. There is, therefore, a twofold reason for examining into the subject at the present time. On the one hand, that in the trials that are just now so prevalent, those exerting themselves in the interests of justice may wield a sharper sword; and on the other that the accused, more likely to be the deserter or bounty-jumper than the true soldier, may not come to harm greater than he in reality deserves.

THE ABSTRACT NATURE OF THE CRIME.

The exact character of rape, as a crime, is not always, even yet, understood. This remark is true, not merely of lewd fellows of the baser sort who may ignorantly render themselves liable to a punishment that in some countries is still death, and in others, as in Massachusetts, would be, in the absence of reprieve, a worse one than death, but of juries and of medical men. In these trials, more than in many others, the issue may entirely depend upon the medical testimony; and in most instances the medical expert has it in his power, where he chooses to lend himself intelligently to the purposes of justice, to point out the existence or the absence of relevant or critical elements to the counsel by whom he is called.

In the legal classification of crimes, rape is enrolled among offenses against the person, the element of violence or compulsion being present, while fornication, or willing intercourse, is only considered an offense against society. The distinction that the law has drawn between the attempt and the completion of the crime, marked, as was formerly the case, by extreme and at times disproportionate differences in punishment, was based not upon the intrinsic harm, moral or physical, to the woman, but, as it were, upon her market value. The view to which I refer is thus stated by Chitty: "An unmarried woman, who has had sexual intercourse, *even by such violence* that she was unable to *resist* with effect, is, in a degree, disgraced, or rather no longer retains her virgin purity in the estimation of society; and there is a natural, delicate, though perhaps indescribable feeling that deters most men who know

that a female has been completely violated, though manifestly after every effort of resistance, from taking her in marriage, but which does not exist, at least in so powerful a degree, if he be certain that the sexual intercourse was *incomplete*. According to the ancient law of rape, which, intending to distinguish between the degrees in the enormity of offenses of this description, made a marked distinction between *ineffectual attempts* completely to violate, and cases where the violation was so complete that the female could no longer be considered in fact a virgin, there was, therefore, required the most explicit evidence of such a *completion* of the offense, that might, under ordinary circumstances, occasion conception; without which proof it was supposed that no man could object to the female as actually contaminated or affected in her virgin purity.”*

The law resting upon this unfounded basis, the fallacy of which is, in part, shown by the well known readiness with which widows obtain a second husband, it was very natural that objections should be made to its modification in accordance with the teachings of medical science. The most unfounded of all these objections seemed valid to the high authority I have just quoted, who was one of the few instances of a medico-legal teacher who has passed the double curriculum of law and physic. A change, he says, “may unfortunately have had the effect of inducing some offenders to *complete the outrage* in cases where, under the old law, the fear of the higher punishment might, especially if opposed with sturdy resistance, have prevented. Under the existing law, capital punishment is the result, although there have been an incomplete assault and the slightest introduction of the male organ *infra labia*, without further perforation or the slightest laceration or actual injury to the vagina, and although seminal discharge be clearly negatived. An offender, under such circumstances, knowing that

* Chitty, Med. Jur., 381. If the argument here advanced were really valid, one ought to hear or see more of a “natural, delicate, and indescribable feeling” that would “deter most” *women* “from taking in marriage” a man who was known or supposed to have previously indulged in coition. It will not do to answer that such facts are never known or discussed by women, for the gossip, everywhere, of good society disproves this; nor is the fact that a woman who has been dishonored, willingly or by force, loses more in caste than an immoral man, alone sufficient to support the view.

he may be equally punished whether he complete his purpose or not, naturally resolves to complete the really greater offense; and thus cases may have occurred where, contrary to the sound policy of legislators in framing an ascending scale of punishment in proportion to the injury or evil to be repressed, the offender is not induced to exercise any locus penitentiæ, but completes what is confessedly a *greater injury*.^{*}

Elwell, who has done more, perhaps, than any one else to establish for medical evidence in this country its due appreciation by the bar, has defined rape as the violent assault (upon a woman, for this is implied by his context) or the overcoming (her) resistance by artificial means, by which (her) chastity is destroyed.† To this definition may be urged the following objections:

1. The crime is not confined to a single sex. It may be committed by females upon males, as proved in repeated instances by the infection of boys with the venereal disease where the lesion was directly traceable to its source.

2. The term *chastity* is uncertain. It may mean one or another of four distinct though correlative states, of which two are moral and two physical.

a. The absence of all previous carnal knowledge or connection whatever.

b. The absence of all to which the party herself was not consenting.

c. The state of virginity, absolute.

d. The state of virginity, relatively to the prisoner alone.

Now the offense may be committed upon a married woman, or even a notoriously unchaste person or prostitute—nay, even upon a woman with whom the offender had himself in times previous been allowed connection by her own consent—although a husband can not be convicted of the offense upon his wife;‡ and, on the other hand, no destruction of any physical sign of virginity is required, nor any further carnal penetration of the female than is implied by the slightest, most momentary, and most external contact of the respective generative organs with each other.

* Chitty, 382.

† Elwell, *Malpractice and Medical Evidence*, 571.

‡ Wharton, *Criminal Law of the United States*, sec. 1131.

3. Violent assault is unnecessary, as with children, or persons whose intellect is permanently or temporarily in abeyance, or those in sleep.

4. Artifice is not required, for similar reasons.

5. Resistance is by no means impliedly present, as in the cases just instanced, and those of excessive fear, or mistake as to identity.

Taylor, the present leading medical jurist of England, defines rape as "in law, the carnal knowledge of a woman (still a woman) by force and against her will."* But there is still a simpler definition. It is as follows: Carnal knowledge *without or against* consent.†

There must, therefore, to constitute the crime, have been, on the one hand, carnal knowledge, and on the other, absence or refusal of consent.

I. What then, in law, constitutes the carnal knowledge of a woman?

It was at one time held in England that, to constitute rape, there must have been an emission of semen within the parts of the female. By the statute of that country now in force, emission is not essential. It has always been held that the entrance of the man within the private parts of the woman, when proved, constitutes rape.‡ In this country it is the general rule that some entrance must be proved, but that there need be neither rupture of the hymen nor emission.§ The English statute is, in the 18th section, thus explicit: "Carnal knowledge shall be deemed complete upon proof of penetration only."|| Does this mean not unless there is decided proof of penetration, or if there is any the slightest proof of penetration? In accordance with such doubt the law was interpreted by one judge as follows: carnal knowledge, *i. e.* penetration, is not complete unless the hymen be ruptured. This would not only rest the crime upon a most uncertain and deceptive

* Taylor, Med. Jurisprudence, 499.

† The above is allowed to be correct by Elwell, but he still confines its application to the cases of women. Loc. cit., 570.

‡ R. v. Allen, 9 C. & P., 31; R. v. Russell, 1 East P. C., 438, 439; R. v. Jordan, 9 C. & P., 118; R. v. Hughes, 8 C. & P., 752; R. v. Sims, 1 C. & K., 393; W. & S., Med. Jur., sec. 432.

§ Elwell, 572.

|| 9 Geo. IV., c. xxxi., sec. 18.

sign, the frequent absence of the hymen even in virgins being now so well established, but it would divide penetration into vulval and vaginal; the former not constituting rape, but a common assault. The majority of judges, however, have not admitted a distinction of this kind. They have strictly adhered to the obvious meaning of the words of the law, and have regarded the rupture of the hymen as in no respect essential. The question of penetration is not for the medical witness, but for the jury to decide from the whole of the facts. In the case of a child, the prisoner was seen perpetrating the act, and though the hymen was normal and unruptured, the crime was yet decided complete.* Actual penetration by an adult without extensive physical injury, must be considered impossible in a child of tender age. The above decision, therefore, settles the true character of the penetration in law, that merely a contact of the genital organs is sufficient fully to effect it.

Upon this point it will be seen that I am at variance with the doctrine still upheld by our medico-legal text-books. I have already quoted Elwell. Taylor, although referring to a case strongly corroborative of my position, yet avoids committing himself upon the subject. Beck avows a doctrine entirely at variance with the knowledge of our time. I have stated the opinion of those best placed to judge, that the presence or absence of the hymen is a most equivocal sign. "I am, however," says Beck, "unwilling to go as far as most of the late writers on legal medicine, who virtually reject it altogether. While it must be allowed that it can very often be destroyed by causes which do not impair the chastity of the female, we are justified, I think, in attaching considerable importance to its presence. It would be difficult to support an accusation of rape where the hymen is found entire."†

Mr. Wharton has expressed himself in similar language: "While the slightest penetration is sufficient, the English practice is decisive that there must be specific proof of *some*.‡ It must be shown, to adopt the phraseology of Tindal, C. J., and afterwards of Williams, J., that the private parts of the male

* Taylor, 539.

† Beck, i., 153.

‡ R. v. Russen, 1 East P. C., 438; R. v. Allen, 9 C. & P., 31; R. v. Jordan, 9 C. & P., 118.

entered, at least to some extent, those of the female.* The law may now indeed be considered as settled, that while the rupturing of the hymen is not indispensable to conviction, there must be proof of some degree of entrance of the male organ within the labia of the pudendum,† and the practice seems to be, not to permit a conviction in those cases in which it is alleged violence was done, without medical proof of the fact, wherever such proof was attainable.”‡

And again he says: “It has been said that penetration may be presumed from circumstances, without specific and positive proof of the organ by which it was effected. This method of proof, however, ought never to be resorted to except in extreme cases, where, from the nature of the case, no other evidence can be had.”§

It is generally ruled, therefore, that proof of some degree of penetration is essential. But how, in the generality of cases, is this fact to be ascertained, save from the testimony of the woman herself, who is often a very unreliable witness? It can not be reached on medical grounds merely, for, as I shall hereafter show, all evidence in this direction may be negative; nor upon the evidence of eye-witnesses, even when they are present, for the exact local relations of the parties are concealed from view, and may, therefore, give rise to mistake as to whether penetration or merely close approximation has occurred.

To this question I may appropriately apply, with only one slight modification, the language of Mr. East: “A very considerable doubt having arisen as to what shall be considered sufficient evidence of the actual commission of this offense, it is necessary to enter into an inquiry which would otherwise be offensive to decency. Considering the nature of the crime, that it is a brutal and violent attack upon the honor and chastity of the weaker sex, it seems more natural and consonant to those sentiments of laudable indignation which in-

* R. v. Russen, 1 East P. C., 438; R. v. Allen, 9 C. & P., 31; R. v. Jordan, 9 C. & P., 118.

† R. v. Lines, 1 C. & K., 393; Wharton & Stillé, Med. Jur., sec. 432.

‡ Wharton, Cr. Law, sec. 1138.

§ Ibid., sec. 1139; Com. v. Beale, Phila. Q. S., Nov., 1854.

duced our ancient lawgivers to rank this offense among felonies, if all further inquiry were unnecessary.* * * * * The quick sense of honor, the pride of virtue, which nature, to render the sex amiable, hath implanted in the female heart, as Mr. Justice Foster has expressed himself, is already violated past redemption, and the injurious consequences to society are in every respect complete. Upon what principle and for what rational purpose any further investigation came to be supposed necessary, the books which record the dicta to that effect do not furnish a trace.”†

Formerly, as I have said, proof of complete penetration was required; now evidence of partial penetration is sufficient. But, as Wharton has admitted in a statement I have lately given, this may be presumed in extreme cases. Such was allowed in the famous Philadelphia case, where a dentist was convicted, probably unjustly, of rape upon one of his patients. Let me prove this assertion by an extract from the judge’s ruling on discharging the motion for a new trial.

“The only remaining question is, whether the evidence given by the prosecutrix was sufficient, if believed, to sustain the verdict. It is true that the Commonwealth failed to produce the corroboratory evidence, which an inspection of the person of the witness and of her garments might possibly have afforded; and it is equally true that we should have been more fully satisfied if such evidence had been produced. There is no rule of law, however, which imperatively demands that the witness shall be corroborated by such evidence. The want of such corroboration is a circumstance to be considered by the jury; and, after being carefully advised on this point by the court, if they regard the evidence produced as satisfactory, the court should not interfere, unless satisfied that their decision was clearly unjust. This we are not prepared to say. The witness, it is admitted, was an innocent, pure-minded girl; she told her sad story with apparent candor; detailed all that occurred from the

* The words I have omitted in the above quotation are the following: “after satisfactory proof of the violence having been perpetrated by actual penetration of the unhappy sufferer’s body.” A proof that, as I have shown, in many undoubtable instances can not be afforded.

† Wharton, Cr. Law, 622.

time the ether was administered; the feeling of her pulse, her arm, her bosom, her person, the fixing of her feet, the drawing down of her body to the edge of the chair, and finally the pain she suffered. It is not strange that the jury believed her, for the question might well be asked: how could an innocent girl detail such occurrences, and with such precision, if it had not really occurred? There is nothing that appears so inconsistent in her story, or so apparently devoid of probability, that a jury should be instructed to disregard it, or that the court should interfere with their decision upon it. But it is said that even if her statement be believed, it shows no legal evidence of penetration.

“It is impossible to lay down any general rules regulating the nature and amount of proof required to establish the commission of such an offense. Each case must be viewed under its own circumstances, and legitimate inference drawn from all the facts proved. Here the witness states the preparation made by the defendant—her feet, which had been crossed, were spread apart, one on each end of the stool; her body was drawn down to the edge of the chair, the defendant was before her, she felt his breath upon her face, which shows that the position of his body must have been leaning over her, and at that time she felt the pain which enabled her to say that she had no doubt that the defendant entered her person. If this evidence as to sensation and position is believed, upon such an issue as here presented of the condition and knowledge of the witness, may not the jury determine from it whether the penetration sworn to was such as the law requires to constitute a rape?”*

The ruling now given is irrespective of the main point in the case, that the patient had been etherized, and was probably laboring under an entire delusion as to the whole matter. It rests the proof of penetration wholly upon the patient's allegations as to preparations made, position taken, and pain endured—all of which do indeed bear directly upon the question of an attempt; but in the absence of corroborative medical testimony, which is often wanting, they are wholly insufficient to prove the fact of penetration, or the effected crime. On the

* Wharton, Cr. Law, 623.

other hand, allowing the truthfulness of the witness, there was sufficient presumption for the alleged relative position of the parties, to satisfy the jury, upon this supposition, that there had occurred a certain amount of penetration, as, indeed, was decided to have occurred in a case I have already referred to from Taylor.

Wharton, while challenging the decision on the ground of possible delusion, allows the point now made. "The general result of both medical and legal opinion," he says, "is that while the learned and able judge who tried the case properly left it to the jury as a question of fact, as he was obliged to do, to determine whether penetration had taken place, the verdict was not sustained by the evidence, and forms an unsafe precedent for the future."*

I agree fully in this statement as regards the merits of the special case involved. When, however, we are told that, save in extreme and therefore very exceptional cases, penetration can not be presumed, "without specific proof of the organ with which it was effected,"† it is at once seen that the prosecution of many otherwise perfectly simple cases ought to fall to the ground. This has, indeed, often occurred, and may again—witness, for instance, the following case, tried in 1844, of rape upon a child, where, as is well known, it is often impossible for any real penetration to occur.

The surgeon here deposed that "the hymen of the child was not ruptured, but that upon the hymen was a venereal sore, which must have arisen from actual contact with the virile organ of a man."‡ Mr. Baron Parke left it to the jury to say "whether, at any time, any part of the virile member of the man was within the labia of the pudendum of the prosecutrix; for, if ever it was, no matter how little, that will be sufficient to constitute a penetration, and the jury ought to convict the prisoner of the complete offense." The verdict was, not guilty.§

* Wharton & Stillé, Med. Jur., sec. 471. † Wharton, Cr. Law, sec. 1139.

‡ I shall, hereafter, call attention to other modes than the usual way by which syphilitic inoculation of an infant's genitals may take place. In the present instance, the verdict seems to have turned upon the question of degree of penetration only.

§ R. v. Lines, 1 C. & K., 393.

The same is shown by another English case, where the judges, Bosanquet, Coleridge, and Coltman, concurred in saying that, "when that which is so very near the entrance has not been ruptured, it is very difficult to come to the conclusion that there has been penetration so as to sustain a charge of rape." There was evidence from the surgeon in attendance that the vulva and vagina were so much inflamed as to render it impossible to ascertain whether or not the hymen remained entire, yet the defendant was acquitted.*

In former times, when it was necessary to prove that the seminal fluid had been discharged within the vagina, it was very naturally allowed that impotence, when existing, should be considered an effectual bar to the consummation of the offense. Now that seminal ejaculation is no longer required, it would seem that the mere allegation of impotence should not be allowed its former protective weight. This, however, is still permitted. "Impotency," says Wharton, "is undoubtedly a sufficient defense to an indictment for the consummated offense, though not for an assault with intent."†

I need merely refer to the facts that the power of erection, and therefore of penetration, may be retained by actual eunuchs, and that some men may be impotent towards one woman, in consequence frequently from some displacement of her uterus, for instance, while perfectly potent in the case of all others, to make the impropriety of longer allowing the excuse alluded to perfectly evident. There are many questions in Obstetric Jurisprudence where proof of absolute impotence, so difficult or impossible to obtain, may be of great importance; but now that conception, or the risk of conception, has nothing to do with the offense of rape, and inasmuch as every woman would suppose from the attempt at coitus that the person attempting at least supposed himself virile enough to complete the act, the shock to her modesty, and the physical violence which she may undergo, alike demand that the law should extend to her the additional protection I have now claimed.

I do not endorse the opinion of Chief-Justice Carlton, that it is always enough if the prosecutrix swear to "carnal knowl-

* *R. v. McRue*, 8 C. & P., 641.

† Wharton, *Cr. Law*, sec. 1135.

edge of her person;”* but I do assert that rape, and attempts at rape, ought not to be separated from each other so distinctly as even at the present day medical jurists would do, and that convictions should occur and penalties be imposed more completely in accordance with the merits of each individual case. This, as I have proved by the late case at Boston, a summary of which has been given, is impossible in Massachusetts, during the continuance of the present statute. That the laws against rape vary in the different states of the Union I shall soon show, and shall point out a grave inconsistency in the New York code. I shall have accomplished good work if I prove to the profession the necessity of insuring uniformity and better justice by their general revision throughout the country. For this, the present time is especially favorable, now that the exigences of returning peace are submitting the statutes of so many of our States to the saving influences of reconstruction.

II. With regard to consent, there are certain parties to whom the word “*without*” directly applies.

These are :

1. Those women who could not possibly have consented for intrinsic reasons—as children of tender age, insane persons, and idiots.

2. Those who could not have consented, for accidental reasons—as those in deep sleep, natural or artificial, from the effect of stimulants, narcotics, or anæsthetics—or in a swoon consequent on disease or wounds, recent, or of long standing, accidentally or intentionally inflicted.

By the former of these classes consent can not be legally given; if granted, it is invalid for removing criminal imputation. By the latter, it would seem necessary for successful defense that the sleep or swoon should have followed, not preceded, the intercourse—an important distinction, as it is certain that with many women of extremely sensitive and irritable nervous organization, loss of consciousness, attended even with hysteriform or epileptic convulsions, may ensue immediately upon or during ordinary sexual intercourse. The support that these accidents, particularly the last instanced, epilepsy, might lend to a criminal charge, is very obvious.

* R. v. Lidwell, 1 McNally's Evid., 606.

As to the other word, "*against*," its character as valid or not in an indictment, will have to be settled by the circumstances of each individual case. The chance of wrongful allegation is so great that these circumstances can not be too closely scrutinized, by the parties on both sides having the case in charge.

The frequency of rape or attempts at the crime is not to be estimated by the number of cases brought to trial. It was formerly everywhere a capital offense. In Massachusetts, the punishment has been lately lowered to imprisonment for life. In New York, the extreme penalty for ravishing a woman in full possession of her senses is only a ten years imprisonment,* while if she has been drugged or otherwise stupefied, so as to be rendered wholly incapable of defending herself, the punishment, strangely enough, though already so slight, is lessened by one-half.† So in England, during the present reign, it has been changed from death to permanent transportation beyond the seas.‡ Judging by the remarkable increase in prosecutions induced in Great Britain by this change, 57 per cent. on the average of four years to 1845, and by official statement in Parliament, an increase of 90 per cent. to 1847, it would seem as though the crime itself were frightfully extending. Moreover, there is no doubt that in many instances the woman's natural shame at publishing her own disgrace prevents disclosure, where there are no other witnesses cognizant of the fact. On the other hand it must not be forgotten that the chance of false accusation, always great, has been infinitely increased now that the death penalty has been abolished. For one real rape, it has been remarked, that is now tried on the English circuits, there are at least twelve pretended cases.

I have already alluded to the danger of these false accusations being brought; this is owing to a variety of causes.

If the charge is made by the woman herself, it may be from

1. An intention to extort money, the technical black-mail;
2. Personal hatred, or sexual disappointment;
3. Unfounded mental delusions, as of disease, the anæsthetic state, intoxication, or sleep;

* 2 R. S., 663, sec. 22.

† Ibid., sec. 23.

‡ 4 & 5 Vict., c. lvi., S. 3.

4. To preserve character, so called, which has been voluntarily endangered.

If brought by a third party, the mother, for instance, the charges may be from the first or last of the reasons just alleged, here implying a conspiracy; or if made in good faith, then in consequence of mistake, depending in the case of a young girl, upon

5. An unnatural vaginal discharge, vulval or other bruise or abrasion, the result of disease, accident or self-abuse.

There is abundant proof that on each of these false grounds very many wrongful convictions have been effected, and that in consequence many innocent parties have perished upon the scaffold.

In these papers, treating as they do of subjects equally interesting and important to lawyers and medical men, I am compelled to cover double ground, and thereby, I trust, while rendering them of practical advantage to either party, to advance the best interests of both. It is requisite that the attorney should know what medical points in prosecution or defense are tenable in law, or are in accordance with facts or equity; and that the physician, who is so often called upon to decide doubtful questions in cases that may never reach the lawyer—often, indeed, to decide whether they shall reach him or not—as well as liable to testify as to fact, presumption or probability upon the witness-stand, should be informed what and to what extent the law assumes and the law requires.

In the first place, it is a presumption of law that innocence is to be taken for granted until the guilt is made to appear by conclusive evidence, so that the burden of proof is necessarily thrown upon the prosecution.

It is therefore always to be presumed that rape was not committed, and that if intercourse were effected, it was with the full consent of the female.

THE PROOFS OF COMMISSION.

I have already defined the character of rape in law. It is necessary then to prove—

1. That carnal intercourse, the so-called penetration, or

rather the reciprocal contact of the generative organs of the two parties, really took place at the time alleged; and

2. That the woman's consent was wanting.

To apply these principles to any given case, it is necessary in a trial for rape to prove, in the first place, that carnal intercourse, as defined above, has been effected.

Besides the allegation of the woman, as I have said, other direct evidence may be present, tending to prove, with the attempt at the act, that mutual contact of the genital organs did take place. Thus, the position in which the parties were found by a third person, or which it is admitted did for the time exist between them, may have been such as would afford very strong presumption that the contact required to complete the crime was really effected. Both parties may have been undressed and in bed together; or the clothes of each unloosened or disarranged, the place they were in under such circumstances being also perhaps retired and the time unusual, or for both these elements of privacy there may have been the use of lock and key—the parties having been surprised, or seen through a keyhole or window.

Such evidence as this, alleged by a third person who is a credible witness, is necessarily very strong. It is almost conclusive as regards sexual connection, but this is a very different thing from forcible and forbidden intercourse or rape. Where such evidence as the above, however, is procurable, the same witness is generally able to testify as to the existence of consent, that is if he or she were present at the time the assault is alleged to have been committed, and not merely immediately subsequently to that event.

On the other hand, if there was no one present besides the parties themselves, it becomes necessary to rest this point of carnal intercourse, so far as direct evidence is concerned, solely upon the testimony of the woman. In these cases, it will be seen, extreme caution is necessary, both regarding her testimony and any presumption from the circumstances attending, which are necessarily in great measure of a strictly medical character.

The medical evidence of carnal penetration without consent may be derived from four sources:

1. Marks of violence about the genitals of prosecutrix or prisoner, from the act itself or its repulsion.

2. Marks of violence on the person elsewhere of prosecutrix or prisoner, from the resistance offered or from general violence.

3. The presence of stains of the spermatic fluid or of blood on the clothes or person of the prosecutrix or prisoner.

4. The existence of gonorrhœa or syphilitic disease in one or both.

It is unnecessary, as I have said, to prove that the vagina had been really penetrated by the virile member, or that seminal emission had occurred within its cavity. The law has fortunately saved us this trouble. Were such proof required, it would have been in many cases almost impossible to furnish, for the following reasons:

1. The valvular membrane at the mouth of the vagina, the hymen, so long considered necessary as proof of virginity, is often congenitally deficient or wholly wanting;

2. It may have been previously destroyed by disease;

3. By accident;

4. By self-abuse, a vice, or rather habit, not at all uncommon even among women;

5. By previous intercourse. When this has occurred, and whether the complainant is a married woman or not—for frequent coitus is by no means confined to those legally entitled to such privilege—the hymen is almost necessarily absent; certainly so, or at least with very rare exceptions, if she has had children at the full time, though the occurrence of an early abortion does not always destroy it.

6. And on the other hand, complete vaginal penetration is by no means necessarily followed or accompanied by intra-vaginal emission of the seminal fluid. It is now notorious that in a large proportion of the cases of illicit intercourse, and indeed, of conjugal intercourse, the completion of the act within the body of the woman is purposely withheld, for the purpose of preventing or avoiding an impregnation, rightfully or wrongfully thought inconvenient, destructive as this method undoubtedly is to the health of both parties engaged.*

* I can not too strongly express my belief that incomplete intercourse,

The law has also wisely refrained from requiring any proof that impregnation has been effected, it being now well known that this is by no means a necessary consequent of the fully completed act—indeed, an unlikely consequent, except at or very near the monthly period of ovulation and menstrual discharge. It was at one time supposed that the fright, disgust and aversion on the part of the woman attending true rape, would of itself be sufficient to prevent conception, and that the occurrence of this was so far proof of consent; but this theory is unfounded—the most frigid and apathetic women being often extremely fruitful, and conception often occurring with vaginal disease that induces great anguish and suffering during coitus.

We will allow then that the four medical presumptions of intercourse, any or even all of them, are present; marks of violence on the genitals or other parts of the person in one or both, stains of spermatic fluid and of blood on the clothes of one or both, and the presence of what are generally supposed conclusive signs of venereal disease. How can these be disproved or rebutted? I shall here deal in general statements, and not descend to the details of proof, some of them microscopic—as for such, accessible in every medical library, we have here neither space nor time.

1. It is of course presumed that an alibi can not be proved.
2. The next point of importance for the defense is to ascertain if the party alleged to have been outraged is really a woman, and on the other hand, if the prisoner is really a man. Both of these questions, it is evident, can only be solved by a thorough medical examination. Their pertinency and validity might easily be shown.

3. Regarding marks of violence. If it were allowed that sexual connection had thus been conclusively proved on the part of one or both persons, it would become a matter of

whether effected by the use of a protecting sheath or by untimely withdrawal, is very detrimental to the local and general health of both men and women. As regards the latter of these classes, I am constantly seeing the effect referred to; as regards the former, I am glad to find my views concurred in by so careful an observer and so competent an authority as my friend Dr. Burnstead, of New York, whose work on Venereal is undoubtedly the best, as yet, in any language.

no little difficulty to prove that this causative connection was between the two parties now in court, or that the marks of violence evidenced were not from recent connection with other parties unknown. Cases illustrative of these positions readily suggest themselves. It is no uncommon thing for such appearances to exist on the persons of women newly married, who, suffering acute physical pain upon the first conjugal approach, at times are inclined to resist completion of the act; and again it is often for the interest of women, married or not, who are yet not at all unfamiliar with sexual intercourse, to play the coquette, and by an appearance of refusal, to conceal the loss of their virginity. In either case the blind fury, so often brutal, of a lustful man, might overleap and violently break all barriers. Suppose, in such cases, an attempt, soon after, at rape upon the woman by another party; or in the latter of the instances suggested, that it is willfully charged upon an innocent person, to give still further color and credit to an attempt at regaining lost repute of chastity—the danger of an unjust conviction becomes very evident. Moreover, these injuries have at times been intentionally and artificially produced for the purpose of giving support to a false charge of the crime.

The marks of violence referred to may be either upon or in the neighborhood of the genitals, or upon other parts of the person. If of the latter kind, it is of as great importance as in the former case closely to scrutinize their character and location, to search for their direct exciting cause, and to decide upon the probable time of their infliction. Thus injuries upon the man, if inflicted by no weapon of defense, would have probably been caused by the woman's teeth or finger nails, or, indeed, if the act is alleged to have been committed in bed, then, perhaps, by the nails of her feet. The probability of their having thus been occasioned, and under the circumstances alleged, can only be reached by closely examining the position and extent of the injuries themselves, and comparing these with the alleged position occupied by the respective parties. If upon the person of the woman, these general injuries would be either from blows for the purpose of stunning or disabling, from efforts to trip or throw her down, from forcible attempts to open her thighs either by a knee or the hands, or from

ligatures that have been applied for the purpose of disabling her, or of compelling a position more favorable for the completion of the felonious assault. The possibility of these injuries having been inflicted, as is frequently the case, by an accomplice, must also be borne in mind. Their date, to a certain extent, is shown by their condition at the time of inspection, the existence of zones of discoloration, etc. And it is evident that any marked discrepancy between the time of their alleged occurrence, and the physical characters that they present, ought, so far, to vitiate the charge.

Thus far the evidence, so far as it goes, tends to the presumption, not that intercourse has been effected, but that it has been attempted.

Injuries upon the genitals are, in the case of either party, much the more important. As I have said, I do not intend here to enter into minute details, and will content myself with merely pointing out generally the more obvious sources of deception or mistake.

Contusions, abrasions, and discolorations around the male genitals, and injuries inflicted upon the organs themselves, are pretty positive evidence to the end borne upon by the class of injuries just instanced, that coitus has been attempted. The local injuries that we are considering are caused by the resistance of the woman, who, in her desperation, has sometimes sliced off the male member with a razor or knife, or even bitten it off. Almost the only successful rebutter of such evidence as this that could be alleged, would be proof of the previous occurrence of some unusual accident to the organs, as striking upon a sharp or projecting body during a fall, or that the prisoner had himself attempted self-mutilation, as is not unfrequently the case during an attack of insanity, or that he had recently been permitted intercourse with some other woman, of very compact genitals, not easily entered.

The result of disease, as we shall perceive, must be borne in mind, and carefully distinguished from the effects noticed.

Local injuries in the female, on the other hand, go further to prove that intercourse has been consummated. They are much less likely to be in the neighborhood of the organs than directly upon or within them, and their presence is in great

measure dependent upon the age of the subject. The younger she is, the more liable to external contusion and to internal laceration and injury, both the outer and the inner tissues being so much the more delicate and friable. If an adult, with whom connection had never by any one been previously effected, there would be a great deal of soreness and stiffness, elements chiefly to be ascertained by the testimony of the party herself, and to a certain extent, by her manner of carriage; and perhaps, though not necessarily, turgescence or abrasion of the external parts, the outer and inner lips of the vulva, the labia, and nymphæ.

The law, properly interpreted, does not require proof that further penetration than this, or even as much as this, has been effected. If such proof, however, exist, it should not be rejected. The remnants* of a recently ruptured hymen, *carunculæ myrtiformes*, as they are subsequently styled, the presence of visible hemorrhage, arising from the outlet or within it, or the detection of the so-called seminal animalcules or spermatozoa within the passage itself, or in the discharges issuing from it, would, within certain important limits, be acknowledged presumptive proof of effected coitus. The presence, however, of mucous, purulent or even hemorrhagic discharge should of itself be allowed to possess little or no conclusive weight.

With a married woman, especially of some little standing as to the time she has held this relation, these signs proportionately fail. If the vulva and vagina have been dilated by frequent intercourse, and especially by the birth of a child or children, coitus being infinitely easier, it is less likely to leave any visible or appreciable trace behind. But upon all these points and in every case that may be brought to trial, it must not be forgotten that however favorable the circumstances, and however consenting the woman, it is always difficult, and in many cases impossible, for the male fairly to enter the vagina of the female without direct manual assistance from himself or from her. In rape, the man can seldom attempt this assistance, both his hands being usually required to overcome the woman's resistance; so that if complete intercourse is alleged, or is proved, or indeed is even admitted by the defense, it might fairly be considered, in the absence of extreme local

injury or complete insensibility on the part of the woman, and this whether she be married or unmarried, as so far strong presumption of her consent, if not direct assistance in the act. To this point we shall necessarily again return.

4. The presence of stains of the spermatic fluid or of blood upon the clothes or person of the prosecutrix or prisoner, has always been thought among the most conclusive proofs. It is easy, however, to show that this is not actually so, except in the case of an infant or a person legally incapable of consent. Putting aside the possibility in microscopic examinations where the clothes, etc., have been soaked and macerated for the purpose of freeing, for more accurate scrutiny, the base of a suspected stain, of error by mistaking minute filaments of cotton fibre, etc., for the true spermatozoon, and, in ordinary cases, of being misled by stains of urine, mucus, or a blenorrhœal discharge, there is still abundant ground for successful defense. While it is not necessary to prove the occurrence of a seminal emission, there must not, on the other hand, be too much weight allowed to the fact of its existence.

We grant that seminal stains exist upon the clothes of the prisoner. What then? With the lower class it is nothing uncommon for the same underclothing to be worn day and night. It is well known that seminal emissions are of constant occurrence during erotic dreams, even among the chastest and most ascetic of men. It is as certain that at times, especially if the person has been of a sensual habit, these same involuntary emissions may take place in the daytime, during strong sexual excitement or desire, without implying sexual contact or even the woman's presence; and that in some men, plethoric or debilitated as the case may be, a similar discharge may occur, either by day or by night, without the consciousness of a lustful thought. Of course, if the stains are upon the external clothes the presumption of intercourse increases. If the seminal traces are detected upon the prisoner's person they prove nothing more, save that the emission was with greater probability or certainty of recent date.

If they are upon the woman's person, their evidence is merely to the same effect, that coitus has been attempted by the prisoner or some one else. If upon or within the genital

organs, there is merely presumption, not positive proof, that complete coitus has been effected, for the spermatic jet, during her struggle, may have been thrown from some little distance, and thus have entered the opening. This supposition, of itself plausible, receives additional weight from the fact that impregnation has been effected, in the known absence of a third party, by men whose appendix was nearly or indeed entirely wanting, or hypospadiac, but their testicles and ejaculatory apparatus yet normal. And, moreover, great care must be exercised that the spermatozoon is not confounded with the *Trichomonas vaginæ*, an animalcule of somewhat similar appearance, though much larger, usually present only in cases of disease, but that may occur, it is alleged, in the vaginal mucus of the chastest virgin.

If the stains are upon the night-dress, they may have been there some little time, and date from some other man than the prisoner. If upon her day-clothing, of course the presumption of his implication is increased, except that, among the lower class, the same carelessness as to changing the underclothes from day to night, obtains as with men.

It will be noticed that the weight of presumption regarding the two parties in court, while coinciding in some points, alternates in others. Thus, spermatic stains upon the *external* clothing of either of the parties, or of both, render it probable that there has been intercourse between them, or an attempt at it. Stains upon the *under* clothing of both parties also imply intercourse of both, probably together. Upon the underclothing of the woman alone, the approach of a man, some man. Upon the underclothing of the man alone, possibly the presence, probably the thought of a woman, either when awake or in an erotic dream. This latter alternative is not, however, absolutely necessary, as in weakened subjects such emissions do undoubtedly take place unconsciously, both by night and by day, just as, on the other hand, the stain may have been effected merely by self-abuse. Upon the *person* of both parties, and, therefore, necessarily quite recent, spermatic stains render the assumption of attempted coitus more certain; effected and accomplished, probably, if the discharge is detected upon or within the organs of the female.

5. The presence of blood stains upon the clothing is of weight similar to the above; much greater if they are coincident, not so great if upon the person of one alone, no matter which of them it may be.

There are but two sources whence recent blood of an at all arterial character, which is that only in any way here conclusive, would be likely, in the absence of a noticeable wound, to stain a man's clothing, except from carnal intercourse. These are some forms of hemorrhoids or bleeding piles, the existence of which could, upon examination, be ascertained with tolerable certainty, and hæmaturia, or a bloody discharge from the urethra, kidney, or bladder; but this, as with that from the rectum, more certainly if from the intestine above it, would be likely to be changed in character, and thus afford quite decisive evidence of its origin. In the absence of either of these alternatives, the blood was probably derived from a woman, but her identity with the prosecutrix ought hardly to be taken for granted, unless she be proved to have been menstruating at the time alleged—in that discharge the blood being ordinarily, but not always, of a more venous character, the coagulum of the uterine excretion being prevented or dissolved by the acid of the vaginal mucus; or to be affected with certain forms of uterine disease, more especially of a malignant or cancerous type; or to have received positive and recent laceration of some portion of the genital organs. The chance that the hemorrhage came from injury inflicted upon the penis during resistance must also be borne in mind. To blood stains on the woman's clothing I have also referred in the above remarks. If coincident upon the clothes of both, the presumption of intercourse, as I have already said, is increased.

6. The last of the several evidences of a medical character usually relied upon in trials for rape, has generally been considered far more certain. It is, however, just as unreliable as the others. I mean the existence of one or other of the three forms of venereal disease.

Respecting their value as signs of recent intercourse, there is still much ignorance prevalent among medical men, but well authenticated cases enable me to express myself very decidedly. The specific characteristics of the three diseases to which I have

referred are now well known; pox, as commonly recognized, consisting of the chancre, a strictly primary lesion, and of syphilis proper, which latter, undergoing a period of incubation at whose close it presents an open sore or chancre, has yet, by primary absorption of the virus, become a constitutional and general disease, while gonorrhœa, or clap, is wholly local in its seat—a purulent discharge from the lining membrane of the genital or genito-urinary canals, and may be initiated and be found contagious in character from a connection perfectly legitimate and without suspicion.

I shall enter into none of the pathological peculiarities of these three affections, or of the differences between them; merely premising that they can not beget each other—that merely syphilis is taken from syphilis, chancre from chancre, and gonorrhœa from gonorrhœa; and that, contrary to usually received opinion, both stages of syphilis, the primary and secondary, the latter in at least one of its forms, are directly communicable, the mucous tubercle occasionally or always engendering a primary sore, the chancre.

If, then, both parties are found to have precisely the same form of disease, the evidence is so far conclusive that they have carnally known each other; whereas, if they are both diseased, but in a different way, the evidence is as conclusive, or even more so, of the other extreme, namely, that though both have become diseased from impure connection, they have done nothing towards infecting each other. This last conclusion can not, however, be considered as positively proved, inasmuch as both syphilis and gonorrhœa may perhaps coexist in the same subject, a fact the possibility of which is yet under discussion.

But how is it when only one party seems infected? and are the essential signs of the diseases in themselves perfectly reliable?

In the case of the male, of whatever age, I have little hesitation in saying that the presence of any form of venereal disease upon the genitals, is so far positive evidence of previous sexual connection, except in cases of inherited taint. It has, however, been observed that Jewish children have received the infection, and presented true chancres on the penis after the division of the prepuce during circumcision, by the organ having been

sucked, to arrest the hemorrhage, by an operator in whose mouth there existed primary lesions.* Such lesions are certainly met with from time to time in the mouths of women of the lowest class, and of men also; how they were originated, there can be but little doubt. If a prisoner admit that he had subjected his organ to such unnatural chance of infection as I have now implied, it is more than likely that he had also exposed himself to it in the more customary manner.

In deciding upon how recent was the connection that had taken place, due care must of course be paid to those medical signs and symptoms decisive of the age of the venereal lesion. On the other hand, it must be borne in mind that all affections of the male generative organs are not necessarily the result of coitus, whether excessive or impure. Some of them are the effect of want of personal cleanliness; others the result of masturbation; or of malignant disease, general, as cancer, or special, as the scrotal disease of chimney sweeps; or of over-fatigue or structural relaxation, as varicocele; or of distant irritation, as infra-Poupertian bubo; or of accidental violence or disturbance, as swelled testicle from horseback riding, exposure to cold, or metastasis from another portion of the body. These and others are points requiring in their solution the aid of the medical man, but it is nevertheless important that the lawyer should know and recollect the fact of their existence. If there is little doubt of the specific and venereal nature of the disease, and there can be little hesitation in their diagnosis by an expert, judgment must not be led astray by unfounded excuses, however plausible. I refer here to the common notion that it is possible to obtain syphilis and gonorrhœa by contact with foul bed linen, or with the surfaces of common privies, as at hotels and the like, where the virus, if deposited, must become neutralized by cold and the action of the atmosphere. After having carefully studied many such alleged cases, I have been compelled to believe that in each and all of them there had been indiscretions by which inoculation had been more naturally effected. I know of no means by which syphilis can thus be accidentally communicated to the male, save the direct ones to which I have already

* Ricord, *Lettres sur la Syphilis*, p. 98.

alluded, and but one by which gonorrhœa, or rather I should say blenorhœa, as the suspicious purulent discharge from the urethra, occurring under such circumstances, should more properly be called. The case to which I refer is where intercourse has taken place while the woman's monthly courses were upon her, the menstrual fluid being at times excessively irritating to the mucous membrane of the male, or while she was suffering from some one or another of the forms of leucorrhœa. Even here, however, such purulent discharge in the male would have been consequent upon coitus, and therefore evidence of its occurrence. I know that it is alleged that the same identical condition may occur in chaste bachelors who have never touched women, but I doubt; though of course allowing for the effects of well authenticated injury, or organic lesion, or constitutional disease.

With the woman, however, there is far more necessity for hesitancy in expressing a decided opinion as to any given case.

1. In the first place, I have no scruple in asserting that gonorrhœa in the female is not to be distinguished by the microscope, by chemical analysis, or in any other way, from purulent discharges entirely innocent in their character, and that may even exist in the chastest virgin, although adult. This remark applies to all stages of the disease.

2. The fact that no external chancre or chancroid is visible, is no proof of the non-existence of actual primary syphilitic disease. In the male, these signs are almost necessarily patent when they exist, cases of such lesion strictly urethral being very rare; in the female, on the contrary, the parts are chiefly concealed from easy inspection, so that the most virulent and infectious sores may be upon the walls of the vagina, upon or within the neck of the womb, or even just internal to the opening of the urethra, requiring, therefore, the most careful examination, especially when the disease is present in the criminal at the bar. It is possible that the newly suggested endoscope may go far to clear up these cases, but it is evident that a drop of pus, or mucus, or serum may here prevent success; an obstacle absent to the ophthalmoscope, and but seldom present in examinations of the larynx.

3. The charge of connection is often made by or in behalf of female children of tender age, even infants. Young girls whose personal cleanliness is neglected, either by themselves or their parents, are not infrequently attacked by ulcerative affections of the external genitals; in some cases slight in character and extent, in others causing their death.

4. Besides such diseases as we have just enumerated, in which the circumstances of a criminal charge would very naturally be suggestive of carnal and infectious intercourse, there is another affection to which young girls are frequently liable, of which complication I have now seen many instances, as also several of the former. A catarrh of the genital organs, consequent upon ordinary exposure to cold, insufficient clothing of the lower extremities, standing over furnace registers, or, in younger children, arising from local or distant and reflex causes of irritation, as after scarlatina, during dentition, or from the presence of ascarides or the scrofulous diathesis, often all of them attended by a purulent discharge from the vagina closely simulating that of gonorrhœa. Convicted upon such evidence as this, more than one poor fellow has been condemned and executed for rape, of which he was undoubtedly innocent. It should never be admitted except—

1st. When the accused party is laboring under a gonorrhœal discharge that existed previously to the time of the alleged intercourse;

2d. When the date of its appearance in the child is from the second to the eighth day after this time; or,

3d. When it has been satisfactorily established that the child had not, previously to the alleged assault, any such discharge. Though, however, any or all of these conditions be present, they do not disprove the innocence of the prisoner, for they may still be mere coincidences, or the child through mistake or design may have accused the wrong person.

5. In the instance of very young female children, who have yet furnished the occasion of very many of the reported trials, there is still another possibility of error, which is no less an important one. I refer to cases where there is undoubtedly present the venereal disease, yet caused by no attempt at sexual intercourse. As this is a matter as yet new, in great measure,

to courts of law, I shall give other and well authenticated instances, in addition to that of the Jewish children, already quoted. In this I am well aware that I am at variance in opinion with Taylor, now the leading medical jurist of Great Britain, who says that, "if a child is really laboring under syphilis or gonorrhœa, this is *ceteris paribus* evidence of impure (carnal) intercourse, either with the ravisher or some other person."* Afterwards, however, he contradicts himself by allowing, in the case of gonorrhœa at least, the chance of other origin, relating the cases reported by Ryan,† where two sisters, one of a year, the other of four, were infected by being washed with a sponge used by a young woman who had a profuse gonorrhœal discharge.

I shall now, however, instance cases of syphilis, or chaneroid, accidentally occurring—a matter of far more importance, as the possibility of their thus being engendered is generally disbelieved.

I have quoted an instance from Ricord. Trousseau saw a little girl of twelve months who had contracted a deep chancre on the buttock. He learned that the mother took the child into the same bed with herself, and as the cold was extreme, pressed it closely to her body to warm it. This woman had primary sores in the vulva.‡ It will be seen, recollecting what I have already said upon this subject, primary sores being only *directly* communicable, that the infection was here clearly by contact, and not by inheritance.

Bertin instances a little girl four months old, healthy herself, as were also her father and mother, who became the subject of a chancre on the upper and inner surface of the left labium.§ It was discovered that an aunt of this child, affected with syphilis, tended and kissed it, sometimes gave it the breast to quiet it, and lastly, that she washed its genital organs with water which she had previously put into her mouth to warm.

Diday treated a lady with a primary chancre on the lower lip, communicated to her by her husband in a way that may be imagined. The mother of a child four months old, she felt it a

* Taylor, Med. Juris., 503.

† Med. Gaz., xlvii., 744.

‡ Gaz. des Hôp., 1846, 571.

§ Tr. de la Mal. Vénér. chez les Enf. Nouveau nez., 77.

great privation to defer until the time when her sore should be healed the kisses with which she had previously and frequently covered it. She one day lost patience, and the result was a deep ulcer upon the labial commissure of the poor child.*

Richet mentions a little girl, born of healthy parents and suckled by a healthy nurse, but yet affected with primary chancres about the anus, concerning the origin of which the medical attendants were much puzzled, until it was ascertained that a clerk of the house, himself infected, had been in the habit of holding the child, bare, on his hands, which were frequently soiled, and which he had not always taken the precaution to wash.

On the other hand, it must not be forgotten that in attempts at connection with young children or virgins, abrasions and excoriations necessarily result, which afford a condition especially favorable to the inoculation of the contagious matter of syphilitic disease. So that in the case of its presence in the prisoner, its absence in the prosecutrix, under such circumstances, would so far afford some presumption of an unfounded charge.

There are still other obscure points in this interesting and important, though as yet little understood subject, the presence of syphilitic disease as proof of special connection—two of which I should do wrong not to mention. They are the facts,

1st. That a woman can be infected vicariously in a variety of ways: through her child during labor, or even before, it having received the poison directly from the father at its conception; by suckling; and, as in the case of the male, by inheritance—the symptoms, save in instances from nursing or infection during labor, being of the so-called secondary type, which, with a single exception, can hardly be considered contagious; and,

2d. That in cases of this special exception just referred to, the so-called mucous tubercle, the disease, although secondary and although, like the chancre, not auto-inoculable by the lancet, is yet directly communicable during connection, and gives rise to the primary lesion, namely, a chancre. This is

* Syphilis in New-born Children, 51.

not owing, as has been supposed, solely to the excess of heat and moisture attending the act, as is proved by inoculation in a similar manner from mouth to mouth. Numerous authorities admit fully, on the evidence of facts, its transmissibility in this manner, by sexual contact,* and it must therefore be borne in mind during our present inquiry.

One other point, though negative, is of such importance that it must not be lost sight of, namely, that connection may be fully effected, one of the parties being extensively diseased, and yet the other receive no infection, nay, even that the disease may be thus transmitted by an uninjured second to a third party. This impunity may be owing to a variety of causes: to precautions taken before or after the act, protective or compensatory; or to unusual toughness of the mucous membrane, approximating in character to ordinary epidermis, from long exposure, perhaps, to external agencies, as in males with a prepuce constantly retracted, more certainly if it is entirely wanting, as with the circumcised Jew; or to idiosyncrasy—just as I believe that the contagious virus of erysipelas, or its congener, puerperal fever, while it ought theoretically to be carried from house to house by every physician who has it in charge, does in reality attach itself to the persons or the finger of certain unfortunates, thus more fully indicating its claims to the title of a private pestilence. This I believe to be as evident as that there are individuals who, from idiosyncrasy, constant or increased by extraneous causes, special or general, debilitating or exciting, are peculiarly prone to receive the disease; a fact that none will deny.

There is a single class of cases, to which it is necessary that I should incidentally allude, those, namely, where the female has been found dead. It would seem that if any marks of violence were present, the charge of murder would take precedence. In a case, however, tried at Edinburgh, the jury convicted the prisoner of rape, and yet acquitted him of the murder, although the proof of the latter crime was much the clearer. The evidence in these cases, in the absence of other personal testimony than that of the woman, must of necessity be wholly circumstantial and presumptive.

* Diday, *Loc. Cit.*, 132.

We have now considered, at considerable length, the various proofs for and against the fact of sexual connection in cases of rape, and incidentally those also which bear upon the existence of the attempt alone. Formerly, before the abolition of the death penalty, the distinction here implied was one of extreme importance, but too often, however, and unfortunately, lost sight of at trials. Whatever opinion may be entertained as to whether or not that penalty was greater than was really demanded by the essential nature of the crime, there is no doubt that many convictions have been effected where the crime, as defined in law, had never been accomplished.

The attempt is now, in Massachusetts, punishable or not to the full extent with its completion, at the option of the Court; in Pennsylvania, imprisonment may be inflicted to the extent of five years and a fine of a thousand dollars; while in New York, the penalties are even less. It may seem, therefore, of less importance to search in every case for such extenuative evidence as I have now presented, were it not that instances are liable to occur where a jury, at all mindful of human infirmity, would be glad to find in the fact that the completion of the offense could not be fully proved, opportunity to ward from the sudden and momentary outburst of passion, provoked, perhaps, by willful temptation from the prosecutrix, the terrible penalty, possible in my own State, and there compulsory where the act has been really effected, of life-long imprisonment.

I agree with Chitty, that "every brutal attack upon a female deserves very severe punishment," and though considering that the offense should be punishable as an injury upon herself and her own moral sense, rather than in deference to "the natural feelings of man and his repugnance to form a matrimonial connection with a female who has been completely violated," yet I do not "hesitate in admitting that an incomplete attempt is not so great an injury as that which, according to the ancient law, must have been completed; and that in legislating, some distinction in punishment should be introduced;"* but I contend that this distinction and the degree of punishment righteous in any given case should be left, more than now, to the discretion of the Court.

* Chitty, 382.

In my next article I shall proceed to the second of the questions involved by the definition of the crime. Carnal knowledge having been effected, was it without or against the consent of the woman?

A New Instrument for Treating Ununited and Oblique Fractures. By E. S. GAILLARD, M.D., Richmond, Va., Medical Director and Inspector in the late Confederate Army.

The treatment of ununited and oblique fractures has always been a source of perplexity and annoyance. In the whole field of surgery, no portion of it is, perhaps, characterized by a more unsatisfactory cultivation and disheartening return.

The literature of the subject alone is material for a volume, though this be but a record of promising suggestions, faithful trials and invariable failures.

The starch and dextrine bandages, plaster-cast boxes, friction and acupuncture, Dumrieher's railway splints, Warren's bow and Maisonneuve's bridge, iron-pins and leaden-wire, Diefenbach's ivory pegs, with the infinite and complex apparatus for extension and counter-extension, etc., have all been admired, praised, tried, and comparatively abandoned.

In a recent number of the London Lancet it appears that this subject, again revived, is engrossing the attention of the most prominent and favorably known medical men in Europe. Fergusson, Bickersteth, Fletcher, Holmes Coote, Hilton and Curling have all recently expressed their opinions and made their suggestions in this connection.

It was from reading the views expressed by these gentlemen that more than incidental attention was again bestowed upon this subject, and that the instrument now to be described and successfully used appeared to promise sufficiently valuable results to warrant its being submitted for examination and trial.

There is a peculiar enthusiasm which usually characterizes the claims and writings of all who submit any original contribution or suggestion for the general good.

This enthusiasm is often a fruitful source of endless errors, and can only be corrected by the caustic touch of analysis and criticism. It is not, however, felt in regard to this instrument, which has been used but once, and though the results were entirely satisfactory, there is a full appreciation of the fact that a single trial is an insufficient basis for either commendation or enthusiasm.

If this instrument answers only as well as many of the complicated pieces of mechanism used in this connection, its simplicity, economy, and easy availability will, it is believed, give it a comparative advantage over other appliances, and will commend it to the attention of those relatively interested. The sanguine expectation is, however, indulged, that this unpretending little instrument will fulfill all of the indications usually manifested in cases of oblique or ununited fracture requiring the use of surgical apparatus.

In so very brief a paper, a description of the condition of the injuries in the treatment of which this instrument is especially applicable must necessarily be avoided. The anatomical relations of the fractured bone, the mechanical problem to be encountered and solved in a successful adjustment of the fragments, the resulting pathological condition of these fragments, when all attempts at adjustment fail, etc., must be omitted.

The chief difficulty to be encountered in the treatment of all fractures is the securing of an accurate and constant apposition of the fractured extremities. If this be successfully accomplished, there is usually complete union, without shortening—the degree of this shortening being a true index of the relative efficiency of the apparatus adopted.

In oblique fracture, over-extension, a frequent fault, results in a false joint or in an entire absence of union. Deficient extension allows an overriding of the fragments with consequent shortening.

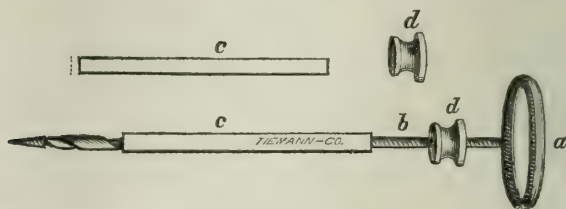
Any instrument which, by keeping the fractured ends in close and constant apposition, will secure the patient from the misfortune of either of the conditions just mentioned, must be regarded as entirely successful.

In ununited fractures, close and continued apposition of the

fragments, with slight stimulation applied to them, is all, so far as is known, that surgery has been able to accomplish.

This instrument, it is believed, will, in its use, fulfill all of the indications specified.

The important fact will be recollected, that in the treatment of fractured bones, the use of steel or wire pins has never produced material local or constitutional injury. All oral and printed testimony tends towards the establishment of this fact.



This instrument is composed of four pieces: the handle (*a*), the screw or shaft (*b*), the sheath (*c*), and the nut (*d*). The shaft or screw is six inches long, the lower part of this being cut with a screw similar to that of an ordinary gimlet, and the upper part being furnished with an ordinary male-screw, on which works the nut (*d*); the shaft being of steel and the nut of brass. The sheath is made of silver and the handle of the instrument of rosewood, or some material equally hard. The sheath is three inches long, and the male-screw, cut in the upper part of the shaft, extends from the handle to a point about two and one-half inches below it. In the plate furnished the sheath and nut are detached, for the more thorough comprehension of the mechanism of the instrument. These pieces (the sheath and nut) are seen elsewhere *in situ*.

It is proper to say that the instrument, with the proportions given, is intended for treatment of a fractured femur; in treating the fractures of other long bones, the length of the shaft and sheath would be proportionally less. The diameter of the shaft is one-eighth of an inch.

The instrument is thus applied—it being supposed that the femur is the bone fractured, and the fragments in apposition.

Through an enlargement of the orifice, caused by the injury sustained, or through an artificial opening immediately over

the site of the fracture, the silver sheath, detached for this purpose, is passed down to the bone. The shaft, with the nut immediately under the handle, is then passed through the sheath, until the point of the shaft rests upon the bone. The operator, seizing the sheath with the left hand, one finger of this hand being under the fragments of bone, slowly turns the handle of the shaft with his right; the sheath should be held firmly to prevent a slipping of the point of the shaft upon the surface of the bone. The process of turning or boring is then continued, until the shaft shall have descended an inch and a half, or until it has pierced both fragments of the bone. The shaft and sheath being then grasped with the left hand, the nut is turned until it reaches the superior extremity of the sheath, and until the resistance offered is greater than the operator can overcome, without using undue force. The forcing down of the nut brings the fragments of bone in apposition, and retains them thus so long as the instrument is allowed to remain—the duration of this period being from three to four weeks for the femur, and a proportionally shorter period in fractures of the smaller bones.

The instrument being *in situ*, all extending and counter-extending forces are removed; light, lateral splints, as a precautionary measure, are then applied, with an ordinary roller-bandage, and the process of adjustment is complete.

The instrument is allowed to remain until there is satisfactory evidence of bony union, when it is withdrawn. Its presence will have been found to cause but little local irritation and no perceptible constitutional disturbance. All who are familiar with the literature in regard to silver sutures, would of course anticipate the fact that the contact of the silver sheath with the tissues produces no material local injury; and those who have seen ivory or steel pins used in the treatment of fractures, will not be surprised at the small amount of disturbance caused by the remaining of the shaft of this instrument in the fractured bone for a period of three or more weeks. As soon as the instrument is withdrawn, the opening through the muscular tissues and through the integuments will heal without difficulty.

It is evident that by the use of this instrument there can be

neither a false joint formed from over-extension, nor the occurrence of shortening from an overlapping of the fragments. This instrument can be improvised by using an ordinary gimlet for the shaft, a portion of a silver female catheter, or the canula of a large trocar for the sheath, and a few bits of pasteboard to act as the nut—these pieces of paste-board being sufficient in number to fill the space recurring on the shaft, between the superior extremity of the sheath and the handle of the instrument, after both fragments of the bone have been perforated.

My experience with this instrument is, as has been mentioned, very limited, indeed; but the instrument is so simple in its preparation and application, and so satisfactory in its results, that it is presented to the Profession for a careful examination and a fair trial.

Case of Tubal Pregnancy. By ROBERT NELSON, M.D., N. Y.

Mrs. K. Y., 38 years of age, lived in a most respectable family as child's nurse more than ten years, not given to company, and particularly chaste in habits, married, in her thirty-eighth year, about three months before the accident that terminated in death. The catamenia had always been very regular until the first week in June, when they ceased. On getting into bed the evening of June 22d, she suddenly felt a sharp pain in the left iliac region, referable to the seat of the ovary, followed by great collapse, cold, clammy perspiration, pale and anxious countenance. She was not seen by her medical attendant before morning, Friday 23d, who at once formed an unfavorable opinion of the case. I saw her at 10 A.M., found her excessively pale, blanched, skin all over cold and clammy, pulse small, very feeble, and frequent; her senses perfect, breathing natural, slight thirst, bowels not moved, nor urine passed since the accident—this was drawn off by catheter by her medical attendant.

In the foregoing mentioned state she continued throughout the day and night, and next morning and day of the 24th only more and more prostrate. I saw her at 4 P.M. Shortly before this visit she asked for a glass of bottled ale, which she relished

and it seemed to revive her; so much so that those about her and herself thought that she was much better, until I dissipated their illusion, saying, to their surprise, that she could not live two hours longer—perhaps not more than one hour. There was no difficulty in arriving at the prognosis, for, although her senses remained perfect and she spoke as in health, yet the collapse and prostration had increased; her breathing consisted of one slow, long inspiration, to end in a short, quick expiration; no pulse could be felt at the wrist or at the humerus, and only a small wave was perceptible at the carotid. After twenty minutes' continuance of this kind of breathing, she drew in one very long and slow inspiration followed by a single quick and short expiration, and ceased to breathe at a quarter past 5 P.M.

In the early period of her illness it was supposed that intestinal perforation and effusion into the peritoneal cavity might be the case, a cause well known to produce sudden and great collapse; but, were such the cause, it would have been preceded by some intestinal disturbance, and would quickly have been followed by great peritoneal pain and inflammation, neither of which symptoms existed. Towards the last it was suggested that an aneurism might have burst; but in this case death would have quickly followed. There remained another idea, entertained from the first, that an ovarian or tubal, or any form of extra-uterine pregnancy had ruptured, or become detached permitting of a slow hemorrhage into the cavity of the peritoneum, this last being the only way of accounting for the sudden seizure and collapse during perfect health, and the collapse and prostration, without peritoneal pain, being kept up for so long a time by a gradual oozing of blood.

On opening the body the intestines were found crowded up, but in no way diseased. Clotted blood filled the pelvis, the iliac and lumbar fossa as far up as under the liver, stomach and spleen. The right fallopian tube was enlarged to the size of a walnut; near to the horn of the uterus the remainder of the tube, over two inches long, with the fimbriæ, floated free in the abdomen. The peritoneal covering of the tumor had given way, as if gradually ruptured, to a diameter of a third of an inch; through this bulged, like a large staphaloma, the foetal

membranes. On opening the tumor, inclosed by a thick decidua and an amnios, was discovered a foetus the size of a bee. The hemorrhage was traced by a clot from the general mass to the projecting chorion or decidua. Both ovaries were natural, and strongly marked with the usual foveæ, said to be the loci whence the ova escape at the catamenial periods. The uterine orifice of the tube was exceedingly contracted. The uterus was two-thirds larger than usual, thickened and lined with a decidua one-third of an inch thick.

It has been noticed that she was suddenly seized with pain in the left ovarian region, a pain that lasted until the morning; but, on examination, no alteration of any kind could be detected there, while the whole trouble was too evidently confined to the right side.

As this woman had led a remarkably chaste life, had enjoyed perfect health, it becomes a consideration how far genetric functions might have been disturbed by copulations so late in life as the 38th year.

Case of Aneurism of the Innominate and Carotid Arteries.

Service of Dr. A. FLINT. Reported by Dr. W. D. F. DAY, House Physician, First Medical Division Bellevue Hospital.

Fred. Wolf, a native of Germany, 50 years of age, and a pianist by profession, was admitted to ward 32, July 13th, 1865. He was a large framed man and in fair flesh, but his complexion resembled that of one laboring under the cachexia of organic, renal or cardiac disease. He was unable to give any satisfactory history of himself, on account of a confused condition of mind and a difficulty in his respiration, which was much increased by mental or bodily exertion. He said, however, that he had been complaining for the last two years, during which time he had had frequent attacks of epistaxis, and that for a year past he had been troubled with a cough. The patient kept his bed from the date of his admission to hospital until his death, a period of thirteen days. He was usually found either sitting up in bed or resting his head upon his arm, the preference being given to the left side.

The cough, which was accompanied by a moderate amount of frothy sputa, was of a peculiar brazen and sepulchral quality, and the voice was grave in pitch and very slightly husky. The respiration was always somewhat labored, but there were at times paroxysms of dyspnœa and complete aphonia.

The pulse, which remained in the neighborhood of 75 per minute, was feeble, and perceptibly weaker in the right radial artery than in the left. The right pupil was slightly more contracted than the left, but the difference was constant.

Upon percussion, the left side of the chest was more resonant than the right. The labored respiration, and the tracheal rales masked the auscultatory signs. From the character of the voice, the dyspnœa and aphonia, together with the inequality in the radial pulses and in the pupils, an aneurism pressing upon the right recurrent laryngeal nerve was suspected. The patient became gradually weaker until the 26th of July, when he died from an attack of intercurrent pneumonia.

An examination thirty-six hours after death disclosed the following appearances:

The pleural and pericardial sacs contained a moderate amount of clear serum. Both lobes of the left lung were in a state of red hepatization, and at the apex of the right lung was a small deposit of tubercle. An aneurism of the innominate and right common carotid was found, measuring five inches in length by three and a half at its broadest part, pyramidal in form, the base directed downwards, and the apex reaching as high as the cricoid cartilage. The aneurism involved the recurrent laryngeal nerve of the right side, and pressed against the trachea, which was flattened from side to side to the extent of half its diameter. The trachea was deflected beyond the median line towards the left side, so that the base of the tumor lay pretty well under the sternum. The base of the aneurism rested upon the right bronchus, but without flattening it. The right subclavian artery was given off from the tumor, and above it was a continuation of the pouch one and a half inches in length, forming the aneurism of the common carotid, from whose apex four arteries were distributed.

On the left common carotid was a small aneurism one inch

by one-half inch, from which four small vessels were also given off. The mucous membrane of the larynx showed some slight congestion, and two points of superficial ulceration.

The large aneurism was lined with fibrin, disposed in layers, leaving a small channel through the centre which accounted for the absence of thrill and murmur and for the diminished force in the right radial artery.

Remarks by Dr. Flint.—This case is of interest in view of the fact that there existed sufficient data for a pretty positive diagnosis, exclusive of the physical signs of aneurism. Aside from the habitual labor of breathing due to pressure of the aneurismal tumor on the trachea, the character of the voice and the intermitting aphonia, attributable to paralysis of the laryngeal muscles on one side, at once suggested the idea of an aneurismal tumor involving the recurrent laryngeal nerve. The absence, at the time the diagnosis was made, of the physical signs of pulmonary disease sufficient to account for the labored breathing, was a negative point of importance; the disparity in the radial pulse between the two sides, and the contraction of the pupil on the right side, were positive points bearing on the diagnosis.

Case of Compound Dislocation of the Tibia Forward. Brooklyn City Hospital. Service of Prof. C. HUTCHISON. Reported by Dr. LEROY MILTON YALE.

Joseph Martin, age 15; nativity, New York; laborer. Strumous diathesis. Admitted to ward 18 about 3 P.M., June 21st, 1864. At 10 A.M. was injured, as nearly as can be ascertained, in the following manner:

While at work in a rope-walk his foot caught in a rope and he was thrown over a "drum." His left leg passed across or into the top of the "drum," the flexure of the knee catching upon its edge, while his body fell outside with powerful extending force.

Examination showed a dislocation of the left tibia forward. There was a wound in the popliteal space about one and one-quarter inches in diameter, through which, on the introduction

of the finger, the condyles of the femur could be felt. The articular surfaces of the tibia could be distinctly recognized in the front of the thigh. The femur was denuded of its periosteum over a space extending from the lower end upward three or four inches, and in width about one-third of the circumference of the bone. The shortening of the limb was one and one-quarter inches. No other injuries were detected save some contusions on the back.

After etherizing the patient the dislocation was easily reduced by passing a sheet under the perineum and tying it to the head of the bed, and then making extension from the foot, while the knee was slightly flexed and pressure made upon the tibia.

A posterior splint was then applied, padded in such a manner that the wound could be dressed without the limb being disturbed.

There was some effusion about the knee, and the sensibility of the foot was slight. The treatment was nourishing and supporting, with anodynes *pro re natâ*.

June 22d. At a consultation of surgeons it was decided not to amputate, a hope being entertained that the limb might be saved.

June 28th.—Suppuration established. Dressed wound with R Kreasoti ʒij., aquae ʒxij. ft. lotio.

July 3d.—Considerable irritation and hectic. Gave quinae sulph. gr. ij., and pil. opii gr. j. every six hours.

July 9th.—Burrowing of pus about three inches above the knee; free discharge of greenish-yellow pus from wound. Put a bandage upon thigh from above downward.

July 15th.—Burrowing about two inches below the knee; roller applied from toe to knee, and bandage on thigh continued. Substituted light side splints for the posterior splint. Kreasote water continued. Patient takes ʒxij. of whiskey *per diem*; beef tea and milk *ad libitum*; pulse small, patient much emaciated; bowels move once a day; has had no chills since admission; no pains in the chest.

July 17th, 2½ P. M.—Died.

The friends removed the body on a coroner's order before an autopsy could be made.

Suggestions regarding Cholera. By A. P. MERRILL, M.D., of New York City.

According to common precedent in regard to epidemic diseases originating in the East, the cholera seems at this time to be tending westward, and in all probability it will again cross the ocean. This must now be considered the common and probable course of all the eastern epidemics which travel westward into Europe. Formerly it was not so. In the infancy of American settlements, and before commerce and travel had bridged the Atlantic with ships, the great epidemics which devastated both Asia and Europe, sometimes extending into Africa, were arrested on the shores of the then almost impassable ocean. Now, the non-contagious epidemics originating in India follow the course of commerce and pilgrimage westward until they are thrown upon the shores of the Mediterranean Sea and its tributaries, thence to be thrust forward toward the setting sun, until the earth is encircled by their ravages. Their strides are often irregular and eccentric, and they are somewhat uncertain in their times, seasons and severities; but in spite of all obstacles and hindrances which science and statesmanship have been able to interpose, their tendency is steadily westward and northward, and hitherto the destruction of life has scarcely been lessened by the observations and experiences of mankind.

The cause of cholera being as yet undiscovered, no effective plan of prevention, either as regards individuals or communities, has been devised; and the disease, like other fatal epidemics, invades both the palace and the hovel, spreading dismay and destruction among all classes. It would, therefore, seem to be the part of prudence, while the disease is traveling toward us, to prepare ourselves for the threatened encounter, and fortify ourselves with all the means of cure at our command.

Since the first appearance of the disease in Europe and America, its pathology has been carefully studied, and whether we agree to call it a peculiar form of fever, or a disease *sui generis*, we know that an early, if not an inceptive condition, is such derangement of the nervous system as results in extensive congestion of the mucous tissues and the skin. Closely simu-

lating the chill of fever in other respects, it differs from it in this: in fever the primary congestions from which, if suffered to continue, we have so much of mischief to apprehend, are located mainly in the parenchymatous structures, while in cholera they are principally confined to the mucous membranes, extending by sympathy to the skin. Extensive and violent as is this morbid lesion, we have no difficulty in recognizing as its necessary concomitant the asphyxied condition of such alarming character, with coldness of the body and breath, while the patient complains, as in other cases of extensive congestions, of sensations of heat and of the painfulness of hot applications. The transpirations of serous fluid from both the mucous tissues and the skin, the painful cramps and prostration of strength, are all legitimate effects of these congestions, and have been noticed as common results of similar conditions of the dermatoid tissues in other forms of disease.

Upon occasions of the appearance of cholera in the south, I have witnessed the best success from a reliance mainly upon strychnia and citric acid as remedies. These are, indeed, the most successful remedial agents in the treatment of diarrhœa, and other diseased conditions attended by watery dejections from the bowels, in some respects resembling cholera. The remarkable power of strychnia in these cases may, I suppose, be attributed to its action upon the organic nerves. But this is an unsafe remedy for popular use, and even physicians often meet with difficulty in its employment on account of its producing toxical effects. The vomiting which attends upon most cases of cholera is generally soon relieved by this remedy, but while it continues we can not know how much is retained in the stomach, and either too much or too little may be given; and when severe congestions are relieved by it, the reaction which follows is mostly expended upon the brain. This it is sometimes difficult to relieve without blood-letting, and great caution is often necessary in the use of this remedy for the purpose, because of the drain which has been made upon the blood by the serous purging and the consequent exhaustion of vital energy. Still, whatever may be the danger attending upon the use of strychnia, its remedial power is such that abstinence from its use can scarcely be justified in any case.

Besides its influence over the functions of the spinal cord and the ganglionic nerves, it gives tonicity to the solids, while the citric acid has a tendency to restore the deteriorated fluids to a healthy condition.

Now I hold—and this is the principal suggestion I wish to make—that my experiments with chloroform administered internally, as reported in the journals and in my published “Lectures on Fever,” prove its great efficacy in all kinds of congestion, except from causes purely mechanical, and that therefore we have abundant reason to hope for successful results from the addition of this to the other remedies mentioned in the treatment of cholera. Chloroform administered internally in doses of a fluidrachm and upwards, and repeated until sleep is produced, may be expected to relieve whatever of congestion there may be in the mucous tissues and the skin, and thus put a stop to the exhausting effects and spasms resulting from excessive serous discharges. It affords us the peculiar advantage, too, of equal efficiency when given by enema in proportionally larger doses. No apprehension need be entertained, if my numerous experiments with the remedy are regarded as of any value, that injury will be caused the mucous membrane, whether given by the stomach or the rectum; and it may be administered by the stomach without the use of bulky vehicles, thus lessening materially the chances of loss by vomiting. In algid congestions, like those of cholera, there is no remedy more agreeable to the patient than pure chloroform, in doses of a fluidrachm. Care should always be taken that it is not adulterated with alcohol, which renders it painfully irritating to the mucous tissues. Without this, the dose above indicated may be frequently repeated in severe congestion, without any inconvenience. When there is no objection to an increase of bulk, the dose is rendered more agreeable, however, by an admixture of water with simple syrup or sugar, made at the moment of administration.

While epidemic cholera continues to baffle the best medical skill, and cause such frightful destruction of human life, I may be permitted to commend these suggestions to the consideration of the profession, both at home and abroad.

PROCEEDINGS OF SOCIETIES.

NEW YORK PATHOLOGICAL SOCIETY.

Stated Meeting, May 24, 1865.

Dr. GURDON BUCK, President, in the Chair.

TUMOR OF THE TESTICLE—DR. L. VOSS.

Dr. Voss exhibited a tumor of the testicle, which he had removed a fortnight before, and gave the following history:

The patient was about 43 years of age, presented a generally cachectic appearance, and complained greatly of pain in his abdomen, which was attended with occasional vomiting and a frequent desire to urinate. He had a tumor on the right side of the scrotum the size of an ostrich egg, oval in shape, and about its middle and outer aspect and also on its lower portion there was to be seen an increased prominence. The tumor was hard, weighty, non-fluctuating, and not translucent. He stated that it had been growing for several years, and denied positively that it had been the result of any injury, or had at any time suddenly increased. The protuberance at the lower portion of the tumor gave distinct evidences of false fluctuation. I suspected that this was the testicle, or rather what was left of it; but I could not give rise in him to any of that peculiar feeling which is experienced when the organ is pressed. My diagnosis then was that it was sarcocele, whether malignant or not, however, I was only able to conjecture. As the patient was suffering very much I proposed to him to have the mass removed, to which he consented.

In commencing the operation I made an incision in the bulge of the lower portion of the tumor, with the intent, if I should find the sound testicle there, to make an effort to save it. On making the incision I found that this false fluctuation, as I had supposed, was due to fibrinous infiltration. On cutting deeper I came on a kind of tissue which certainly did not resemble the tissue of the testis; I thought it was then a fibrous tumor, and resolved to remove it, which I did without any trouble.

After its removal, when I cut into the tumor, I found that it was an hematocele which contained a fluid like thick chocolate. The reason why I had not detected the testis was this: instead of coming on the proper tissue of the testis, I struck the epididymis, the sound tissue of which I did not recognize as belonging to that part where I had expected to find the testicle; this latter organ was at the end of the

tumor looking with its face upward, and tucked away in a very small compass. The tunica vaginalis was very much thickened, which accounted for the want of translucency referred to.

Under the circumstances the testicle was removed with the mass. If the proper nature of the tumor had been recognized in time to save the testis, I do not think that the patient would have been able to stand the suppuration which would have been necessary to close such a large opening as would be left. The scrotum was not adherent to any part of the tumor.

This variety of hematocele, which is not of traumatic origin, is very well described by Virchow in his late work on tumors. The wound is now nearly closed and the patient is doing well. In conclusion, Dr. Voss remarked that the error in diagnosis which had been made by him was not infrequent.

Dr. BUCK stated that he could recall one or more instances in which he had not diagnosticated that condition.

Dr. SANDS remarked that such cases would suggest the expediency of using an exploring needle in every case of scrotal tumor in which a diagnosis is not very certain. He saw a mistake committed at Bellevue Hospital a few years ago which impressed him with the need of this caution. A man was admitted with supposed hydrocele. He was brought into the Governor's room, it being a public day of some sort, and was examined by quite a number of surgeons, including the resident and visiting staff, and they all were of one opinion, viz., that the case was one of hydrocele, except one gentleman, who, having his doubts as to the existence of true fluctuation, suggested the propriety of introducing a trocar. The suggestion was not heeded, and a cut was made with a view to evacuate the contents of the sac; but the knife entered an encephaloid tumor, and the patient had to be taken immediately to a ward in order to have the mass removed. He had several times tapped scrotal tumors in doubt, but had never had occasion to regret the operation. The sense of fluctuation under such circumstances could not be relied upon, inasmuch as the same condition of things existed in encephaloid tumors. He remembered a mistake which Nelaton had committed in the case of a swelling of the arm. Nelaton told the class which was assembled at his clinic of the difficulty of distinguishing between solid and fluid tumors, and mentioned the fact that soft solids gave the sense of fluctuation; but that in the case before them, although there was fluctuation present, the tumor was evidently adipose. After having given a very interesting lecture upon the subject he cut into the mass, when, alas! it turned out to be an *abscess*.

Dr. Buck remarked that the feeling of fluctuation was very deceptive as to the knowledge which it gave of the existence of pus or other fluid.

He remarked that his attention was called, a few days before, in a patient suffering from another trouble, to a tumor of the shoulder, which was believed to be a case of cold abscess. He was struck with the sense of fluctuation which it communicated. It occupied the anterior half of the deltoid muscle, was flattened and nearly circular; but he noticed that in stretching the skin over it that at its circumference there were to be seen one or two superficial sulci running towards the centre and crossing it. This circumstance pointed out the case at once as one of fatty tumor. He remarked that fatty tumors generally consist of large masses which are separated by superficial sulci, and, ordinarily, when the integument is loose over them, they can be brought into view. But in that case there was another feature which determined that the tumor was not an abscess, its mobility upon its bed, there being no attachments for it below, and the fingers could be inserted some distance under it.

A few years ago, continued he, I made a mistake in supposing a cold abscess, situated below the edge of the angle of the scapula and underneath a layer of muscle, to be a fatty tumor. It was flattened, indolent, and slid easily under the surface; but when it was cut into, the matter which it contained was evacuated, proving conclusively the error in diagnosis which had been made.

Dr. Voss stated that Pirogoff had written a very interesting paper on the subject of fluctuating and non-fluctuating tumors, in which were detailed many mistakes of his own and others.

Dr. Buck said that Dr. Alex. H. Stevens used to relate an instance which came under his notice when visiting Edinburgh in the day of Mr. Liston. That gentleman called Dr. Stevens' attention to a tumor of the nates, and expressed a confident opinion as to its nature. When every thing was ready for the operation, Dr. Stevens suggested the propriety of introducing an exploring needle; the suggestion, however, was not heeded, and the operator came down upon a cold abscess.

SEPARATION OF THE EPIPHYSES OF THE FEMUR—DR. LITTLE.

Dr. LITTLE presented a specimen of separation of the epiphyses of the lower end of the femur, and gave the following history of the case:

Willie Bull, aged 11. On April 18, while hanging on the back of

a grocery wagon, his right leg caught in the spokes of the wheel and was suddenly twisted around, and he sustained a separation of the epiphysis of the lower extremity of the femur. On being called to see the patient a few minutes after the injury, I found the upper fragment projecting about three inches through an opening in the soft parts on the outside of the upper part of the popliteal space. The finger could be passed upward and downward for a considerable distance, showing considerable contusion and laceration of the soft parts, in the neighborhood of the wound. No communication with the knee-joint could be discovered. The patient was etherized, and, assisted by Drs. Church and J. L. Smith, I reduced the deformity, and placed the limb on a double inclined plane. There was a cartilaginous but no bony crepitus. There was at this time no pulsation to be discovered in the posterior tibial artery, and the leg was cold.

April 19th.—Consultation with Dr. Parker. Patient was again etherized, and the injury thoroughly examined. At this time the warmth had returned to the limb, and pulsation of the posterior tibial could be felt. It was also found that during the night the upper fragments had become displaced, and that they could be kept in proper position only by the leg being flexed upon the thigh. As it was deemed best to give the boy a chance to save his limb, the leg was secured in this position by a band of adhesive plaster around the ankle and over the upper part of the thigh, and then allowed to recline on a pillow. The patient suffered considerable from the irritative fever and from swelling about the joint, which began to subside in about a week.

On the tenth day the patient had a slight hemorrhage from the wound. On the thirteenth day, about 10 in the morning, while dressing the wound, a severe secondary hemorrhage took place, which prostrated the patient very much. This was arrested by pressure over the femoral, and a tourniquet applied as soon as it could be obtained. Amputation of the thigh was then decided upon, as the soft parts in the neighborhood of the wound were in such a bad condition that it was deemed useless to attempt to find the bleeding point.

At 8 in the evening reaction had taken place sufficiently to warrant the operation, and I amputated the thigh at a point a little above the middle. There was but little hemorrhage at the time of the operation; patient rallied slowly from the shock. The wound did well, and the patient is now, twenty-four days after the operation, able to go about on crutches. The wound has almost entirely healed.

An examination of the limb showed that the soft parts were in a state

of decomposition—the hemorrhage proceeding from a wound of the anterior tibial artery at its origin from the popliteal.

The knee-joint was involved in the injury; the synovial membrane was intensely reddened throughout the joint. The bone presented almost a perfect separation of the epiphyses.

He also, in this connection, presented the lower end of the humerus, with a portion of the epiphysis, which he had removed from a patient of Dr. Livingston. The injury which occasioned its removal was a fracture which extended half through the epiphyseal line, and then across the lower extremity of the bone higher up. The patient recovered with a stiff arm.

Dr. Buck remarked: I think it is not unusual where a separation of the epiphysis takes place to find that the separation is not entirely confined to the epiphyseal line of junction; there is generally a small chip which separates besides. I am reminded by Dr. Little's case of one that I can not, however, give sufficient particulars of to have much weight. It occurred in the practice of the late Hugh Walsh, of Fordham. The patient was a boy, aged 14, whose limb was also twisted in a revolving wheel by being caught in the spokes. My recollection is not sufficient to state whether the fracture was compound or not, but I recollect that there was a separation of the epiphyses.

Dr. Voss stated that he had met with a case of the same character as that presented by Dr. Little. The fracture was not compound at the time of the accident, but subsequently an abscess formed and the upper fragment protruded. In that condition the doctor saw the case, and was compelled to amputate. He also stated that he had met with another instance of this separation of the epiphysis at the lower end of the tibia, in a boy about 14 years of age, who fell down from the first story of a house, and caught his foot between two pieces of wood. The upper fragment protruded through the skin, was reduced, and the limb placed on a double inclined plane. A portion of the epiphysis became necrosed, and was afterwards removed, but the boy finally recovered with the use of his joint. In conclusion he stated that the separation of the epiphysis from the diaphysis was most frequent, as the result of inflammatory processes—in that case the separation always occurring before advanced age.

Dr. Buck stated that when such a separation took place the inflammation was generally acute—at least such was the experience at the New York Hospital.

BRAIN, IN A CASE OF MENINGITIS—DR. A. JACOBI.

Dr. JACOBI presented a specimen of a brain removed from a man who died at the age of 45, and gave the following history of the case:

I know very little of his previous history, except that he had not been frequently sick, and had been leading a rather irregular life. He was a short, stout man, with a short neck, and had exhibited very violent temper all his lifetime, especially during the last years of his life. His temper would sometimes be so violent that he would treat his family not only badly, but cruelly. About a year and a half ago he was taken with a severe attack of bilateral pneumonia, so diagnosed by the physician then in attendance. From this attack he recovered after some time, but not without developing some unusual cerebral symptoms. He was once, during this attack, taken with a violent convulsion, which lasted for an hour, followed by coma, which lasted from twelve to twenty-four hours. He recovered from this slowly, and, it was thought, completely; but it was remarked that his temper grew more and more violent from day to day. About a year ago he was taken with another attack of what appears to have been pneumonia. He recovered from this also, when he commenced to show symptoms of insanity. He would, for days and weeks, appear quite rational, when of a sudden he would exhibit some curious mental phenomena. Being a barber, he would sometimes leave his customers in their chairs, partly shaved, and walk off to sit down on the stairs in an adjoining hall. This mental condition increased to such an extent that he was compelled to give up his business and move to another part of the city. When he arrived at his new home his forgetfulness appeared to increase; he asked his son if all the baggage on the walk belonged to him, and when so informed, he made known the fact to all the other tenants; he also seemed very curious to know what kind of a house he moved to, and so on. It was thought, subsequently, best to remove him to a lunatic asylum. I called to see him, but could not convince myself of his being insane. He talked very rationally; said that he had no confidence in physicians, talked a great deal concerning his household affairs, and, as far as I could see, there was no insanity in his case, at least at that moment. I went off, promising to return another day, when I hoped to be able to detect the existence of his mental trouble. The next day there was a fire in the neighborhood, which he attended. He stood for a while with his head uncovered, looked at the fire, after which he turned round in the direction of his residence. When he arrived there he was evidently bewildered, and commenced to gallop off in a given direction, as if suddenly conscious

of his whereabouts. He was watched by those in the house, but he disappeared.

The next that was heard of him was that he had been taken with convulsions in Centre street, and a policeman, who supposed him to be intoxicated, conveyed him to the Tombs, from which place he was afterwards transferred to the New York Hospital, where I saw him the next day. I was told by the house physician that the patient came in with fever and with all the symptoms of inflammation of the brain, and also had a slight injury on the left cheek. When I saw him the other day his cerebral irritation had disappeared; there was very little fever, his pulse being 106 and pretty regular, and there was only one direct cerebral symptom present, and that was dilatation of the left pupil. He was unable to speak, and remained in a stupid condition. In that condition he continued for another day. He had been very violent before that, so that it was necessary to confine him in a straight-jacket. Two days and a half after he was received into the hospital he was attacked with pleurisy of the left side, and died twenty-four hours after the diagnosis was made.

I was kindly asked by the house staff to be present at the autopsy. There was an abscess in the lower lobe of the right lung, with a pretty thick pyogenic membrane. There was a hemorrhagic effusion in the middle lobe of the right lung. The middle and lower lobe of right lung and lower lobe of left lung, especially the anterior portion of it, were very much congested. In one portion of the middle lobe of the right lung there was a condition which nearly approached that of gangrene, giving forth an intensely foetid odor. The right heart was full of blood, the left heart was entirely empty. The brain showed evidences of an old and also a recent attack of meningitis. There was inflammation not only of the dura mater but of the pia mater and arachnoid. The gray and white substance of the hemispheres of the cerebrum, the cerebellum, and the medulla oblongata was found in a very hyperæmic condition. There was a moderate amount of liquid in the arachnoid sac, but the principal lesions were those which appeared to be of older date. There was a yellowish-greenish discoloration of the whole arachnoid; this latter membrane, too, was so thick that the convolutions of the hemispheres seemed to be flattened in consequence. When the specimen was fresh the exudation was so thick that it presented the appearance in some parts of an uniform smooth surface. There were also evidences of thickening and adhesion of the filamentous tissues between the dura mater and arachnoid. Further,

there were little granular bodies to be seen in great numbers near the adhesions between the dura mater and arachnoid; these bodies were also found in large numbers in the choroid plexus of either side. There was no liquid in the lateral ventricles.

I forgot to state that he had another convulsion during his second attack of sickness, about a year ago.

These apparently tubercular bodies in the arachnoid and choroid plexuses are not tubercles, but are found, on microscopical examination, to be composed of nothing but connective tissue, with a number of nuclei. How old this first attack of meningitis was I am unable to say, but the last attack, of which he died, was evidently of not more than three or four days' duration. The old exudation appears to date back to his first attack of pneumonia. It is very common, especially in children, to see brain symptoms supervening on inflammations of the lungs, and under those circumstances, too, you meet with these tubercular bodies on the arachnoid which, as I have said before, are composed entirely of connective tissue. I should have stated that there were some old adhesions between the left pleuræ, and there was, also, about seven or eight ounces of liquid in that cavity, thus justifying the diagnosis of pleurisy already referred to.

CRANIOTABES—DR. JACOBI.

Dr. JACOBI also presented the cranium of an infant who died the day before at the age of a little more than five months. He then gave the following history:

Some five days ago I was called by Dr. Downs to see this infant with him, and on examination we found that the child was apparently in a good condition, and its weight was not below the average of children of that age; but its skin was pale and *sallow*, and it evidently had more fat than muscle. There were eczematous eruptions over the face and part of the head; there was some hair on the anterior portion of the cranium, and but very little on the posterior portion. The complaint was that the child for the last fortnight had a large number of convulsions, which were of longer or shorter duration. The parents stated that after such attacks the child would sleep a little, then have a drowsy spell, and finally wake up. There were no cerebral symptoms manifest when we examined the child together. The pupils seemed to be normal, there were no contractions, and there was no paralysis. The mother stated that the convulsions would commence in what appeared to be an attack of laryngismus stridulous; the child

would become pale, then purple, would be unable to breathe for a time, then a slight interrupted inspiration would take place, resembling a whoop, when the patient would either come to or go into a convulsion. An examination of the lungs and of the thymus body did not offer any explanation for the symptoms. There was slight dullness over the upper portion of the sternum, but certainly not enough to justify hypertrophy of the thymus body. When we examined further we found that the posterior portion of the cranium was easily impressible with the finger; it seemed to have no more resistance to pressure than thick paper, and, when it yielded, would give a crackling sensation. The anterior portion of the cranium was nearly in the same condition. The large fontanelle was very large and the sutures were evidently not very firm. Neither the liver nor spleen was enlarged. The epiphyseal extremities of the tubular bones were enlarged. Our diagnosis was *craniotabes*, that is to say, rachitical softening of the cranial bones, with the cotemporary changes in the brain which almost always accompany such a condition.

We thought if the child could live a month or two in order to allow time enough for the general improvement of the system, recovery might take place; but it died a few days after in a convulsion.

On examination after death, the *dura mater* was found tightly adherent in the situation of the *lamdoidal suture*. The upper portion of the occipital bone and the lower portion of the parietal bones have been removed, and, on holding them to the light, there is evidently a great many places in which there is apparently no osseous tissue whatever—especially is this the case on the right side. I forgot to state that the occiput of the child appeared at one portion to be flattened; this condition can now be appreciated in the general configuration of the bones. The right parietal is evidently the one which is most affected; the left parietal bone shows a number of very distinct softened spots in which there is no appearance of bony tissue. Through one spot, where I had previously removed the *pericranium*, I was enabled to-day to see a large letter. There was softening of the cerebral substance, and there was, further, some effusion in the *arachnoid sac* which, judging from the symptoms, must have occurred during the last days of life. There was a little effusion in the spinal canal, which would flow down into the cranium when the child was turned over. Otherwise there was nothing abnormal. The lungs were quite healthy, the heart normal, and the liver very *anæmic*. The spleen exhibited a very small polypus-like exeresence in the middle of its external aspect, which was quite pedunculated, but which, on examination,

proved to be nothing else but spleen tissue. The normal appearance of the lower portion of the intestinal canal was somewhat different from what we usually find. As a general thing the rectum of the infant and child extend upward, and a little to the right, then turns over entirely to the right ilium, there forms a curvature, then turns over in a straight line to the left side, forms another curvature, and then takes its course upward. Here there was nothing found except the normal curvature, as is seen in adult life.

There is one other point to which I wish to refer, and it is in connection with the parietal bones. The periosteum was very easily removed from these bones, which is probably due to the fact that the external lamina was not fully developed.

A case of this nature (craniotabes) has been presented to the Society by Dr. Krackowizer, in which the softening was confined to the same set of cranial bones. The position of this softening is explainable from the fact that those bones which are its seats are most exposed to pressure in lying. In conclusion, he stated that he once met with craniotabes in a foetus, and in that instance, there being no particular portion of the cranium exposed to pressure, the softening was general.

CANCER OF THE BREAST—DR. BRADLEY.

Dr. BRADLEY exhibited a tumor of the breast which he had removed from a female aged 56 years. She did not present the usual cancerous cachexia, and the tumor appeared, with the exception of one or two small glands in the axilla, to be of a local character. These glands were removed with the tumor. The disease first made its appearance about a year ago, and towards the last increased quite rapidly. It was confined mostly to the lower and outer border of the gland, and occasioned a retraction of the corresponding side of the nipple. The texture of the tumor was very tough and cried under the knife, and, on microscopic examination, was found to consist of cancer-cells and nuclei.

INFLAMMATION OF THE DUODENUM—DR. LEWIS SMITH.

Dr. LEWIS SMITH presented a specimen taken from a child who died at the age of 10 years. He had been in good health up to the time of his last sickness, which commenced on the 2d of May. He first complained of pain in his bowels, and this pain continued during the whole of the following day, though he was able to get about the house. On the third day he ate his breakfast as usual and went to his business down town; before night, however, he returned, feeling chilly,

and complaining of the return of the pain in his bowels. He had no appetite for food, went to bed, and was feverish. The family being in moderate circumstances did not call a physician, and the patient continued in the condition described during the two following days, Friday and Saturday. On Sunday he was noticed to be in a slight degree yellow, and he then commenced to vomit blood, which continued during Monday and a part of Tuesday, and which was estimated in quantity equal to a pint. Dr. Smith saw him for the first time on Tuesday, and found him then with a very sick stomach, very tender abdomen, and with tenderness along the spine. His bowels were constipated, and he was very much reduced, partly on account of the blood which he had lost and partly on account of the pain. He died within the next twenty-four hours, and, on making the post-mortem examination, the thoracic organs were found healthy, and the stomach, which was distended with gas, was found to contain also some blood.

On following down the alimentary canal, the mucous membrane of the duodenum, below the situation of the common duct, was found very much thickened. Below this thickened portion the membrane seemed healthy. There was a dark substance occupying the jejunum, which appeared to be decomposed blood. The liver was about the usual size, and, upon examination under the microscope, was found to contain a little more fat than usual. The membrane lining the common duct was inflamed, a little above its point of junction with the duodenum, for a space of about two or three lines in circumference. There was no noticeable disease in the gall bladder, and, from the absence of any distension, it did not appear that there had been any obstructions in the common duct sufficient to dam up the bile.

BULLET IMPACTED IN A METACARPAL BONE—DR. GURDON BUCK.

Dr. Buck exhibited a specimen of impaction of a bullet in the lower half of the metacarpal bone of the middle finger. The patient was a soldier, who was shot at by a guard while making his escape from a train in motion. This occurred some months ago. The metacarpal bone at the seat of the injury was enlarged, and the opening which existed there communicated with a rough surface. The joint was somewhat rigid, allowing only of a limited motion. A second opening existed a little below the articular extremity of the first phalanx, where it was represented to Dr. Buck that a buckshot had been removed. The existence of the ball impacted in the bone was not known, and the patient being anxious for the removal of his finger, an

operation was commenced to remove dead bone, and resulted in the production of the specimen referred to.

FRACTURE OF THE THIGH—DR. BUCK.

Dr. BUCK presented a second specimen, which was one of fracture of the thigh at the junction of the shaft with the condyles, which resulted from the patient, a vigorous cartman, thirty or forty years of age, being thrown violently from his cart on his left knee. Twenty-four hours after the injury Dr. Buck saw the case; it was then reported to him that there was a difference of an inch and a half in the length of the limbs, and that crepitus had been felt at the point of fracture. The limb presented a very considerable degree of swelling of a uniform character; there was no remarkable increase in the breadth of the limb, and the parts around the fracture seemed to retain their normal contour. On handling the parts, the false points of motion were readily recognized by those who had made the first examination. At the time Dr. Buck made the examination no crepitus was discoverable. The question in regard to fracture of the condyles was duly considered; but on account of the extreme tension of the soft parts which came on soon after the accident, no lateral motion was then obtainable between the fractured portions. The existence of a fracture separating the condyles from the shaft was obvious enough, and there was an ecchymotic discoloration upon the anterior aspect of the limb. The patient died a week after the injury, with delirium tremens. Upon examination of the limb after death it was found that the shaft had been separated from the condyles in an oblique plane from below, upwards, and from before, backwards. The extremity of the upper fragment was very sharp, and from its extreme obliquity caused a fracture of the inner condyle. The two condyles were separated from each other, and the inner condyle had attached to it about five or six inches of the shaft. The pulsations of the posterior tibial artery, strange to say, were not interfered with by this remarkable fracture.

Dr. BUCK remarked that it was very easy to see how such a sharp fragment in the popliteal space might perforate the artery, and also how easy it would be for the sharp edge of the upper fragment to splinter, and afterwards perforate the quadriceps tendon. Both those conditions were obtainable with such a fracture, and when the latter existed, the two portions of the split tendon straddling the upper fragment would necessarily interfere with union.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Lectures on the Diseases of the Stomach, with an Introduction on its Anatomy and Physiology. By WILLIAM BRINTON, M.D., F.R.S., Physician to St. Thomas's Hospital. American Edition. Philadelphia: Lea & Blanchard, 1865, pp. 302.

Diseases of the stomach are popularly supposed to be more frequent in the United States than in any other country in the world. Whilst we are disposed to doubt the correctness of this belief, we can not deny that the class of affections in question plays a very important part in adding to the discomfort of our people and in swelling our mortuary records. It is somewhat strange that no one of our own physicians has seen proper to give us a monograph upon so fruitful a theme as that of stomach diseases; but in the absence of such a work we welcome with great pleasure the very satisfactory treatise which comes to us from Dr. Brinton's pen.

The work consists of an introductory chapter, in which the anatomy and physiology of the stomach are fully dwelt upon, and of eight lectures, devoted to the consideration of the functional and organic diseases to which this viscus is liable. Dr. Brinton gives it as his opinion that the normal acid of the gastric juice is the hydrochloric, and that lactic acid, when found in this secretion, is a secondary product derived from the food ingested. In this view he is certainly sustained by the weight of evidence.

The lecture on "Ulcer of the Stomach" is especially full and interesting, and the remarks on the treatment are characterized by much good sense and sound reasoning. Dr. Brinton lays more stress upon diet than upon medicines: of the latter he regards opium as the most valuable; mercury he strongly condemns. Relative to it he says:

"It would be a dereliction of duty in me not to express the strong opinion I entertain against the employment of mercury in these cases, in any form and under any pretense whatever. I believe that I have known one or two instances in which the ulcer has been definitely produced by the administration of mercury for other maladies, and am certain that I have witnessed relapses which could only be attributed to a similar cause. A single calomel purgative has even appeared to undo all that months of sedulous treatment had been able to effect towards the relief of a gastric ulcer."

Dr. Brinton has not a great deal to say relative to dyspepsia—that wide-spread affection of civilized society. He, very properly, considers

that in the great majority of cases the stomach is not primarily at fault, and yet it is doubtful if he gives sufficient prominence to the nervous origin of the disease. We all know, from experience, how greatly the function of digestion is influenced by the mental organization of the individual, or by some temporary derangement, or shock received by the nervous system. We should have been glad, therefore, had he considered the matter to a greater extent from a psychological point of view. His description of the disease is, however, graphic and full, whilst at the same time it is sufficiently concise.

Relative to the treatment of dyspepsia, Dr. Brinton appears to have no small amount of faith in drugs. Pepsine has, however, disappointed him; tonics he has found generally useful, especially quinine, and the preparations of iron, zinc and bismuth; of the forms of iron he prefers the citrate. Aperients he regards, when properly used, as of the first importance, and sedatives can scarcely be dispensed with. The benefits to be derived from diet and regimen, though mentioned, are not, we conceive, sufficiently insisted upon. Mineral waters are extolled in a few brief words, but physical exercise and the proper regulation of the intellectual faculties are not even referred to as curative agents.

Upon the whole, Dr. Brinton's book is a very excellent treatise, which should be in the hands of every medical practitioner. We regret, however, that the American publishers have brought it out in a style so inferior to that of the English edition. The paper is thin and dusky, and the whole "getting up" of the volume is of the cheapest and most common description. Physicians are just as fond of typographical comforts as other people, and are fully as willing to pay for them, too; and yet American medical books are, many of them, printed in a style that would disgrace a yellow-covered romance of the flashiest kind. Some two or three of our medical publishers manufacture their works in a highly creditable manner, and doubtless find that they thereby advance both their pecuniary interest and their professional reputation. Messrs. Lea & Blanchard have frequently shown us that they know how to get up handsome books; and therefore we have the less patience with them when they give us one which does them no honor by its appearance, than we should have with those from whom we never have had a fine-looking volume, and from whom, therefore, we expect nothing. A book that is worth printing at all, is worth printing well. When our medical publishers uniformly act upon this maxim, it will, we think, be more advantageous for them, as it certainly will be better for the readers.

A Dictionary of Medical Science; Containing a Concise Explanation of the various Subjects and Terms of Anatomy, Physiology, Pathology, Hygiene, Therapeutics, Pharmacology, Pharmacy, Surgery, Obstetrics, Medical Jurisprudence, and Dentistry; Notices of Climate and of Mineral Waters, Formulæ for Official, Empirical and Dietetic Preparations; with the Accentuation and Etymology of the Terms, and the French and other Synonyms, so as to Constitute a French as well as English Medical Lexicon. By ROBLEY DUNGLISON, M.D., L.L.D., Professor of the Institutes of Medicine, etc., in the Jefferson Medical College of Philadelphia. Thoroughly revised and very greatly modified and augmented. Philadelphia: Blanchard & Lea, 1865.

This elaborate title relieves us from the necessity of giving an account of the extent and value of the work offered by Prof. Dunglison to the profession in this new edition of his Medical Lexicon.

It would be a work of supererogation to bestow a word of praise upon this Lexicon. We can only wonder at the labor expended, for whenever we refer to its pages for information we are seldom disappointed in finding all that we desire, whether it be in accentuation, etymology or definition of terms.

The constant introduction of new terms into scientific language renders a revision of a medical lexicon frequently necessary. In the second edition of "Dunglison's Dictionary," as it is familiarly known, several thousand terms were introduced which were not to be found in any other medical lexicon in use in this country. Each subsequent edition has been augmented by a large addition of new subjects and terms, and although it is but a few years since the last edition appeared, we are informed by the editor that over sixty pages of new matter have been incorporated in the present one, the whole having undergone a thorough and complete revision. In fine, there is no more complete or better medical lexicon extant.

The Medical Register of the City of New York, for the year commencing June 1, 1865. Published under the supervision of the New York Medico-Historical Society. GUIDO FURMAN, M.D., Editor. New York: 1865.

The profession of this city is certainly indebted to the Society under whose auspices this volume is issued, and to the editor under whose immediate charge it has been carried through the press, for a valuable work.

It contains an account of the various Medical Societies of this city,

as well as the American Medical Association, and the State Medical Society, and gives the names of the officers and members of the city Societies. It also gives a brief record of the several Colleges, Dispensaries and public charities, with a list of the Faculties, Officers, and Trustees of the same.

A few pages are devoted to the record of some historical facts, among which we find a brief account of the origin and history of Dispensaries. This is followed by a necrological record for 1862-65. The book finally closes with a directory of all duly qualified practitioners of medicine in the city and county of New York, the address and office hours of each physician being given. This department of the Register could be very materially increased in value, practically as well as historically, if the date and place of graduation of each physician, with the titles of works or monographs written by him, could be added. As it is, we hope the enterprise of the publishers will be sustained, for we have found it a very useful and convenient book of reference upon local medical affairs.

PROGRESS OF THE MEDICAL SCIENCES.

I.—MATERIA MEDICA AND THERAPEUTICS.

1. *Effects of Iodide of Potassium.*

In a communication to the editor of The Lancet, Mr. Hodges makes the following statements:

I was attending the butler of a gentleman in my neighborhood about six years ago, and found it necessary that my patient should take iodide of potassium, and after taking it for three or four days he had as pretty an attack of erysipelas of the face as I ever saw in my life, which, strange to say, he himself attributed to the medicine I had given him. After this he accompanied the family to town. On his return some time afterwards, I was again attending him, and again gave him iodide of potassium, and with exactly the same result, an attack of erysipelas. Having never seen erysipelas follow upon taking iodide of potassium before, I could not quite make up my mind that the drug was really the cause of the two attacks, but thought that each time it might have been simply a coincidence, and resolved, in order to satisfy myself, to try it again, which I did after an interval of several months. He then took the medicine for nearly a fortnight, and, when I was beginning to be satisfied the former attacks were not due to the iodide, he was again attacked with erysipelas of the face, scalp, neck, &c., rather severely. I may, however, mention that I have frequently had the same patient under treatment for various

ailments, and on one or two occasions I have given him alkaline medicines, (liq. potassæ, &c.,) and each time they have caused him much discomfort; for though he has not had another attack of erysipelas, he always complains of great heat and irritation of the skin generally, and now he does not forget to remind me, when about to prescribe for him, not to give him any of that "potash stuff."

With regard to the effects of iodide of potassium on the uterus, I may mention that several years ago I had under treatment a young woman, of rather easy virtue, for general debility, anæmia, and amenorrhœa. Her general health was completely restored by the use of tonics, steel, &c., but the catamenia did not return. She was not pregnant. Some time afterwards it became necessary to give her iodide of potassium for several weeks. During that time she menstruated imperfectly once; but, though several years have elapsed, she has not, as far as I know, menstruated since.

Within the last six months a young girl, aged thirteen, was under my care for chronic rheumatism. She had been regular for several years up to a few months before. I gave her iodide of potassium, which, although it did not cure her rheumatism, had the effect, after taking it a week, of re-establishing the catamenia, which have since been regular.—*London Lancet*.

2. Remedy for *Hydrophobia*.

"The Messenger de Provence" promulgates the following remedy for hydrophobia, which may not be perfectly new, but is at least remarkable for extreme simplicity. In the year 900 of the Christian era a mad bear, rushing along the banks of the Saône, was attacked by a number of watermen, about twenty of whom were more or less severely bitten. Six of these men became affected with hydrophobia and were smothered; the remaining fourteen, who leaped into the river and swam across, escaped scatheless, having been saved by contact with the water. The anonymous author of this story, comparing the virus of vaccine with that of rabies, contends that if the punctures of vaccine are immediately washed with fair water, no effects are perceptible from the operation; and that if the bite inflicted by a rabid animal is also at once washed with cold water, no further consequences need be apprehended from the injury. It remains to be seen whether this is a legitimate comparison, and whether it is an ascertained fact that lotions of cold water have the power of neutralizing the effects of vaccination.—*Journal of Practical Medicine and Surgery*.

3. Powdered Talc as a Dressing.

Dr. Gouyon recommends powdered talc (silicate of magnesia and alumina) as a dressing for burns. It is, he says, unaffected by temperature; when applied, it calms pain, cleanses the wound, and excites a rapid growth of healthy granulations. It is also applicable to wounds of any kind and in any situation; and it may be combined, according to the indications, with chloride of lime, tannin, alum, calomel, quinine, etc. The powder is dusted over the part with a dredge. Talc is also, Dr. Gouyon says, an excellent styptic in cases of venous and capillary hemorrhage; such as the bleeding from leech-bites and epistaxis.—*Gaz. Méd. de Lyon*.

4. *Solution of Venice Turpentine as a Dressing for Wounds.*

* Alluding to M. Nélaton's approval of camphorated spirit as a dressing for wounds in his wards, M. Kerner states that this has the disadvantage of being expensive and easily evaporable. He states that, being surgeon to the Messrs. Dolfuss' factories at Mulhouse, where from 3,000 to 4,000 work people are employed, he has ample experience in treating all kinds of wounds, and finds the following a most valuable as well as a cheap application: Venice turpentine, 2lbs. 8 oz. troy; bicarbonate of soda, $6\frac{1}{2}$ drachms; water, 18 pints Imperial. This is to digest in a bath for six days, at a temperature not higher than 75° C., and then filter. It constitutes a highly soluble turpentine soap, which is very cheap, and evaporates slowly. The cicatrization of even very large wounds under its use is wonderfully rapid, the secretion of pus being very slight, while there is an entire absence of foetidity. A compress, consisting of eight folds of linen, well soaked in the solution, is applied over the whole breadth of surface, and left on (covered with oiled silk) for twelve hours, being moistened by means of a sponge with some of the solution every four or five hours. Granulations speedily shoot forth, requiring to be touched with nitrate of silver; but the discharge is quite insignificant, so that none of the encumbering apparatus required when cerates are used need be resorted to. When the wound is accompanied with crushing or contusion, leading to the fear of much inflammation, it should be exposed to a jet of cold water for some days before employing the turpentine.—*Bull. de Thérapeutique.*

5. *Danger of Subcutaneous Injections.*

Professor Nussbaum, of Munich, has just published an interesting account of an accident which happened to himself. Suffering from neuralgia, he had injected morphia under his own skin more than 2000 times—sometimes to the extent of five grains of morphia in twenty-four hours. Two months ago he injected two grains of acetate of morphia dissolved in fifteen minims of water, and accidentally sent it direct into a subcutaneous vein instead of into the cellular tissue. He gives a graphic account of his dangerous position for two hours, after which the effect passed off. He has seen similar effects in a smaller degree in two of his patients, and the practical lessons are, that as it may be impossible to avoid veins at all times, and one may be punctured unawares, subcutaneous injection should **always** be done *very slowly*. The effects are so instantaneous that the syringe can be stopped at the first sign of danger, and some of the injected fluid mixed with blood may even be sucked out again by the syringe. It is very remarkable how the effects of the same dose of the same substance differ when directly injected into a vein and mixed with the venous blood, and when they filter into the blood from the cellular tissue through the unbroken coats of the vessels.—*Medical Times & Gazette.*

6. *A Few Words Against the Habitual Use of Purgatives.* By C. B. RADCLIFFE, M.D., F.R.C.P., Physician to the Westminster Hospital, and to the National Hospital for the Paralyzed and Epileptic.

Not a few persons still look upon purgatives as necessities of life. They seem to think that their bowels would never act without the customary pill or potion. They stare incredulously if they are told that

constipation is the natural result of some error of diet. They scout the idea that in some cases the bowels are habitually constipated with advantage and not with disadvantage. Nor can it be denied that the conduct of not a few medical men is such as to encourage rather than to contradict these notions.

I object to the habitual use of purgatives on two grounds chiefly. In the first place, I object because the object in view may be attained by a more ready and rational way; that is, simply by regulating the diet in a proper manner. In the second place, I object because the very cases in which purgatives are habitually resorted to are very often those in which the constipation which it is intended to remove is a state to be encouraged, rather than a state to be corrected. To show fully why I make these objections, is not possible in the few minutes at my disposal. I can only hint at the reasons which have influenced me; and this, in fact, is all I propose to do.

1. Man is an omnivorous animal. Originally it was not so. Then, "every herb bearing seed which is upon the face of all the earth, and every tree in which is a fruit of a tree yielding seed," was ordained for meat. Now it is very different; and it is to the butcher, rather than to the gardener, that man looks for his daily food. And what must be the consequences, so far as the bowels are concerned, of so doing? What must be the consequences, in this respect, of taking too much animal food, and too little green meat? These are questions, to answer which it is not necessary to look further than to the illustration supplied in the case of an herbivorous animal, and in that of a carnivorous animal—the cow, for example, and the lion. The cow grazes on grass, and has a semifluid motion every hour or thereabouts. She is, so to speak, in a state of habitual diarrhœa; and this is her natural state. The lion, on the other hand, gorges on flesh. He eats seldom, and his bowels are relieved of a mass of little more than dry earthly matter once a fortnight, or even not oftener than once a month. He is in a state of very decided constipation; and this state is natural to him. As, therefore, the food of man departs from that of the lion, and inclines to that of the cow, it may be expected that his bowels will act more and more after the fashion of the cow; and so, in fact, it is. Supply green meat—salad, fruit, and the rest—and the bowels, as a rule, will act well enough: withhold it, and constipation will be the result. This is my experience in this matter. At any rate, this much I may say, that I have been always able to dispense with the habitual use of purgatives by attending to this point, and to that which I have next to consider.

In regulating the bowels, it also seems to be important to bear in mind the composition of the natural food provided for the state of infancy. This food, of course, is milk. Now milk, especially human milk, contains a large quantity of cream—contains a large quantity of that ingredient which is carefully extracted in too many instances from the milk which too often has to do duty for mother's milk. And what is it which this cream is intended to do? Part of it, it is probable, has to be burnt in the respiration, in order to keep up the heat of the body. Part of it, it is possible, has to act as food for nerve-tissue, by supplying some of the oily matter which is an essential ingredient in this tissue. And the rest—what of it? It is possible—nay, it is probable—nay, rather, it is certain—that it will further the secretion of bile; for it is one of the functions of this secretion to dispose of unused-up "respiratory fuel." Hence, to feed an infant on skimmed milk may lead of necessity, as one of its consequences,

to constipation, by stinting the natural outflow of bile into the bowel; and hence the natural remedy for the constipation thus arising may be, not "to seek to regulate the secretions," but simply to follow Nature's lead, and restore to the milk used as food what had been robbed from it—namely, its cream. Surely, in theory, it is better to be *secundum naturam* than *secundum artem*; and most assuredly all I have seen in practice has taught me to prefer cream to gray powder, or any other abomination of the kind, as a means of correcting the constipation of infants. Indeed, my own experience amounts to this: that sickly, spoon-fed infants have improved in health, and ceased at the same time to be sources of solicitude as to the condition of their bowels, as soon as care was taken that the milk used in their food was not deficient in the natural amount of cream. And, if this be so with infants, why should it be otherwise with persons of maturer age? If this be so with cream, why should it not be so also with butter, fat, and oily matters of other kinds? In theory, I can find no reason why it should not be so; and still less can I find a reason in practice. Practically, indeed, I can say this without hesitation: that the result of insisting upon the addition of a due amount of oily and fatty matter to the food, together with (at proper ages) a due amount of green meat, has been to enable me to dispense, for the last dozen years at least, with the habitual use of purgatives in practice.

In properly regulating the bowels, there are, of course, many other things to be attended to; as, for example, the proportion of bread and potatoes in the food; the question of brown bread *v.* white, and coffee *v.* tea; of alcoholic drinks *v.* water; of exercise *v.* rest; and so on. But all these, as I believe, are of minor moment only, when compared with the two questions which have been considered. Indeed, I have no hesitation in saying this broadly, as the result of an experience extending over at least a dozen years, that in any ordinary case, without making any other change, the habitual use of purgatives may be dispensed with by taking care that the diet contain a sufficient amount of oily and fatty matter, together with a sufficiency of green meat in those cases in which green meat is not contra-indicated by age or by some other circumstance; and that in every case the result is most unmistakably beneficial.

2. A sentence or two will serve to say what can now be said in justification of my second objection to the habitual use of purgatives. I objected on the ground that the very cases in which purgatives are habitually resorted to are very often those in which the constipation which it is intended to remove is a state to be encouraged, rather than a state to be removed. The cases I had in my mind in making this statement are cases of old age, and of debility generally—cases in which, most assuredly, constipation must be looked upon as the rule, rather than as the exception. Now, in cases of this kind, certain things may be assumed. It may be assumed, that the digestive powers are feeble. It may be assumed, that debility of digestion implies inability to deal with the more innutritious kinds of food, green meat especially—with those kinds of food, that is to say, which are favorable to frequent stools. It may be assumed, also, that debility of digestion implies slowness of digestion; and that one effect of slowness of digestion is to lessen the number of stools. It may be assumed, in fact, that constipation, within certain limits, ought to be the rule in the cases in question, if the diet be of the kind best suited to the wants of the system; and that it is simply foolish to attempt to bring about a contrary state of things. In the cases in question, indeed,

to cause the bowels to act every day, after the manner of many, is, as it seems to me, to do what must have the effect of starving the system, by removing the food from the bowel before digestion and absorption have had time to do their work—is to victimize the patient as Sancho Panza was victimized by his pretended physician at Barataria; that is, to take away the dish before time had been given to partake of it. At any rate, be these reasonings right or wrong, of this I am sure in practice—that good results from disregarding in a great degree the habitually constipated condition of the bowels in many cases of debility, of old age especially; and that to attempt to remove constipation by the habitual use of purgatives in these cases, is only to increase debility and irritability.—*British Medical Journal*.

7. A New Poison.

A new poison has been examined and reported on by M. Pelikan of St. Petersburg. The plant which produces it comes from the Gaboon, and is used by the natives of that locality for poisoning arrows. It belongs to the natural order *Apocynaceæ*, and produces its injurious action almost exclusively upon the heart. M. Pelikan's experiments were made upon the common frog, and they have led him to the following conclusions:

“1. The poison produces at first an increased action of the heart. 2. After a while the pulse decreases in frequency, and the heart soon entirely ceases to beat. 3. The cessation of action is not regularly progressive. 4. When the ordinary action of the heart has been completely arrested, the ventricle still exhibits peculiar movements, which appear to be peristaltic. 5. When the ventricle has completely ceased to act, and is strongly contracted, and empty, the auricles, though full of blood, continue to contract. 6. Finally, the paralysis of the heart has nothing in common with cadaveric rigidity. When once paralyzed, this organ does not respond to any stimuli, whether mechanical, chemical, or electrical, applied either directly or to the nerves.”—*Medical Press*.

With reference to the subject of Prof. Pelikan's researches, we would state that several years ago Drs. William A. Hammond and S. Weir Mitchell published the results of their investigations relative to a new poison, corroval, the effects of which upon the heart were identical with those ascribed by Prof. Pelikan to his poison. The papers, two in number, were entitled: “Experimental Researches Relative to Corroval and Vao, etc.”—*American Journal of the Medical Sciences*, July, 1859. “On the Physical and Chemical Characteristics of Corroval and Vao, etc.”—*Proceedings of the Academy of Natural Sciences of Philadelphia, Biological Department*, p. 4, 1860. Both these papers are also contained in Dr. Hammond's *Physiological Memoirs*. It is very probable that the active principle of the poison studied by Prof. Pelikan is identical with *Corrovalin*, the alkaloid extracted by Drs. Hammond and Mitchell from corroval.

II.—PATHOLOGY AND PRACTICE OF MEDICINE.

1. *On the Treatment of Fever.*

“With respect to the treatment of fever,” observes Dr. Wilks, “I adhere to the old practice of the hospital—that which was laid down by Drs. Bright, Addison, and Barlow, in their lectures, and which they adopted for so many years with success. The teaching of these professors was to the effect that a large number of cases ran their course without any other treatment than careful watching and feeding; that they required no other medicine than a simple saline; but that some needed a stimulant during the progress of the disease, and others required it from the commencement. The question, therefore, with regard to the administration of stimulus, was *when* to give it, and in what quantity. At the present time there are advocates for a universal method in favor of alcohol in all cases of fever, just as there are those who indiscriminately administer ammonia in scarlet fever, and who, when failing to prove its value in all cases, fall back upon the explanation that if alcohol or ammonia be of real service in a bad case of fever or scarlatina, and if these remedies do no harm in the milder forms, it is a good rule to administer them universally. Such a method is not only unscientific, but I believe positively injurious; for in many cases of typhus fever in young people, where the brain has been involved, I have a very strong opinion that the brandy which I have seen given in such cases has been positively hurtful. In my intercourse with medical men, I judge that very many are scarcely alive to the fact that typhus fever is very rarely fatal in young persons—the prognosis, indeed, only becoming serious as years are added to the age of the patient—and, therefore, that they are too apt to attribute recovery to their remedies. Young persons always do well if left alone. Of this fact I could now quote a large number of cases in proof; and, on the contrary, the few instances which I have seen end fatally have been those in which a large amount of stimulus was given from the commencement of the disease; and, what perhaps is even more to the point, the withdrawal of stimulus in some cases where it was adopted as the method of treatment, has been attended with the most decided advantage.

“Although the subject of fever and its treatment may appear exhausted, yet this difference of opinion respecting the use of alcohol shows that this therapeutic struggle must continue for some time longer: for whilst we are witnessing the free use of stimulants in fever, we are reading that it was Dr. Graves’s plan to administer antimony in the same disease. When deliberating on the merits of such contradictory treatment, many escape the dilemma by believing that different diseases have been the subject of treatment, and that fever has changed its type; they assume, without even a skeptical thought passing through their minds, that the favorable issue was due to the remedy, and therefore their conclusion is a logical one. Such persons, however, have no right to frame this or any other opinion as to treatment unless they are conversant with the natural history of the disease; for, did they quite realize to themselves the fact that in young persons typhus fever is rarely fatal, they might with great advantage state the argument in another form; as, for instance, Will a few doses of antimony, or a few daily ounces of wine, or the abstraction of a few ounces of blood, be sufficient to kill a patient suffering from a disease the tendency of which is to subside spontaneously in the course of a few days? Surely, too, every medical

man must have seen cases, either under his own charge or his neighbor's, where, from a mistaken diagnosis of the nature of the case, a remedy has been given which, above all others, he would have discountenanced had his opinion been otherwise. He must, for instance, have seen several grains of opium administered daily on the supposition that the case was one of delirium tremens instead of fever, and yet the patient has done well. If he saw many such instances, he might consider that he had good reason to believe in another change of type. Without, therefore, denying that fever may annually change in character as to some minor features, yet I believe that the argument of its great alteration, founded upon the administration of remedies, to be a most fallacious one.

"I would not wish to dictate rules of treatment to any of my medical brethren; but my own opinion is that expressed in the first few sentences of these observations. I believe that support and a moderate amount of wine is the best treatment; but I assert that alcohol is not an antagonist to the fever, which runs its course in spite of the administration of the stimulant. I would not say that in many cases of typhus in young people a little wine may not be useful; but the fact still remains, which can not be gainsaid, that such cases would do well without any stimulant whatever. When the ground is thus cleared, we shall know better what we are doing. I might mention that the only two cases which I have seen fatal of late have been those of two students, to whom a large amount of stimulant was given, and who had the care of the most assiduous nurses both night and day. In one of these there were constant convulsive movements during the last five days of life, with coma vigil, and other symptoms resembling those in which the late Dr. Graves would have administered antimony. In this case there was no albumen in the urine, nor in another, which recovered, where convulsions were most violent. Albumen has, however, been very frequently present in other instances.—(*Lancet*.) *Half-yearly Abstract of the Medical Sciences, etc.*

2. *On the Treatment of Malarious Fever by the Subcutaneous Injection of Quinine.*

Mr. Moore states that he has lately employed, with great success, the hypodermic method of administering quinine in the treatment of malarious fever.

"I use," he says, "the strongest solution of quinine which can be prepared, viz., thirty grains of quinine, ten or twelve drops of sulphuric acid, and half an ounce of water. Of this, previously strained, I inject from half a drachm to a drachm, the former quantity containing somewhat less than four grains of the active agent. With the exception of a little sulphate of soda, if the bowels are confined, I have used no other remedies in complicated cases of any type of malarious fever. When the spleen is enlarged, or a leucocythemic condition manifest, I prescribe, as an additional curative agent, one or other of the preparations of iron.

"I generally inject beneath the skin of the outer belly of the triceps, and sometimes over the deltoid. The operation, however, is equally effective on the thigh or calf; and in cases of large spleen, the action of the remedy appears intensified by injection over that organ. I use a small glass syringe furnished with a silver point, and introduce the latter beneath the integument for half an inch or less. The pain

is not more than the prick of a needle, and indeed is often less objected to than the bitter taste of quinine. I have never observed the slightest irritation following the operation, excepting when performed with a small trocar and common glass syringe; and when quinine has been used in *suspension*, and not in *solution*. I therefore insist on the use of a proper instrument, and on perfect solution of the alkaloid. If the quinine is not invisible the preparation is unfit for use; the syringe becomes clogged, and the areolar tissue does not appear able to absorb the solid material, which, hence, creates irritation.

"The best time to inject is shortly before the expected cold fit; but it may be done during the first stage with the effect of lessening and occasionally stopping the paroxysm. When an accession is expected during the day, injection in the morning will, almost invariably, prevent the attack.

"In cases of remittent I have endeavored to inject during the remission; but do not wait for this period. In severe cases, the injection should be repeated at intervals of four, six, or eight hours.

"I believe four or five grains of quinine, injected beneath the integument, are equal in their effects to five or six times that amount taken into the stomach; also that the results are more certain, and that relapsing attacks will be found to be less common; while the economy of the treatment is self-evident.

"I have now injected a considerable number of cases in the European General Hospital, Bombay, and elsewhere, and find the number of those who lose their fever after the first injection is upwards of 60 per cent. of all classes of intermittents. Some cases, however, require two, three, or four injections, and remittent attacks a larger number."—(*Indian Annals of Medical Science.*) *Half-yearly Abstract of the Medical Sciences, etc.*

3. *On a New Lesion of the Brain in General Paralysis.*

Dr. Baillarger, on examining the brain of insane individuals who had suffered from paralysis, noticed long ago that, in some cases, when the gray substance of the anterior lobes, after the removal of their membranes, was scraped with the back of a scalpel, prolongations or ridges of the white matter stopped the knife, and were cut away with some difficulty. The white matter could be thus exposed, and looked of a firm consistency and somewhat yellowish. Some of its prolongations, elastic and tough, resembled the epiglottis in color and appearance. In other words, a process of induration seemed to have gone on in the most superficial layers of the medullary substance, whilst the deeper ones retained their normal consistency.

M. Regnard endeavored to ascertain whether this lesion was constant, or at least frequent, and whether, in all cases, it was possible to make out its presence or absence. He made, for this purpose, twelve post-mortem examinations: in eight, he found the lesion well marked; in three, it was indistinct and incomplete; in one, it was totally absent. In five of the eight cases in which the lesion was well marked, the disease had not lasted more than nine months—a fact which shows that this lesion exists in the first stage of general paralysis. In 20 of the same class of cases, the white matter was softened beneath the superficial layers that were indurated. The obvious conclusion is, that the lesion is entirely restricted to the superficial layer of the white matter. In the cases comprised in the second category, there was very evident softening of the white matter; whilst in the single

case of the third series, there was not softening only, but actual disfluence of the white matter. It would appear, therefore, that the lesion is all the more marked, in proportion as the brain substance is of diminished consistency. All the patients whose brains were examined had, during life, suffered from embarrassment of speech, and M. Regnard draws attention to the fact, long ago pointed out by Bouillaud, that the anterior lobes were diseased; and also that the third left frontal convolution was, in all the cases, as much disorganized as the rest.—(*Annales Médico-Psychologiques*.) *Half-yearly Abstract of the Medical Sciences*, etc.

4. *On Sclerosis of the Lateral Columns of the Spinal Cord, in an Hysterical Woman whose four Limbs had, during Life, been permanently contracted.*

This case formed the subject of a paper read before the Société Médicale des Hôpitaux, of Paris, 25th January, 1865. The author began by stating that, up to the present time, the alteration known by the name of sclerosis, or gray degeneration of the spinal cord, had been only studied in cases of progressive locomotor ataxy, and in that complaint the lesion is almost exclusively limited to the posterior columns of the cord, leaving the anterior and lateral columns in a nearly normal state. In the present case, on the contrary, the posterior and anterior columns were healthy, whilst the lateral columns, on both sides, for a great portion of their thickness and throughout their length, from the medulla oblongata as far as the lumbar swelling, were the seat of the gray degeneration. Several of the anterior roots were also atrophied, but the posterior roots were all normal. There was no trace of spinal meningitis. The characters of the sclerosis were very distinct, the columns had a grayish, semi-transparent, gelatiniform look; their consistency was greater than usual; their substance was infiltrated with a transparent amorphous or fibrillary substance, through which were scattered nuclei of connective tissue and corpora amylacea. Lastly, there was atrophy of the nerve tubes, which presented a series of dilatations and constrictions. The gray matter was healthy. The nerve cells were normal, as in the cases of locomotor ataxy, previously examined by Drs. Vulpian and Charcot.

Sclerosis of the lateral columns has been recorded several times, among others, by Dr. Türck, (Academy of Sciences of Vienna, 1856.) Dr. Charcot once met with it in a case, the history of which he could never learn. This lesion, therefore, is not altogether rare, and more carefully made post-mortem examinations will, doubtless, multiply instances of this new pathological species. Up to this time, the cases on record have been chiefly deficient in accurate clinical observation. The present case of Dr. Charcot's, however, is less deficient than the rest in this respect.

The patient had, from the age of 14, presented the most characteristic symptoms of convulsive hysteria. Later, the convulsive fits became less frequent, and were replaced by permanent motor disorders. Thus at the age of 34, after an hysterical fit, she suffered from contraction of the left arm and leg, which lasted a fortnight, and then disappeared all of a sudden. A year afterwards the same limbs became contracted, and, after a time, the right limbs were similarly affected. For two years she was obliged to remain almost completely motionless, with scarcely a few short intervals of inter-

mission. At the end of that time, a marked improvement set in spontaneously, and the patient was able to walk about and attend to her house. In 1855, however, another violent fit brought on contraction again of all the limbs and the muscles of the trunk. From that time the condition remained persistently until 1864, when an inter-current affection carried her off. Her intellect was never impaired, up to the very last.—(*Gazette Hebdomadaire de Méd. et de Chir.*) *Half-yearly Abstract of the Medical Sciences, etc.*

5. *Double Amaurosis. Cure coincident with the Expulsion of a Tænia.*

A mason, 28 years old, was treated in 1861 for tapeworm. Being subsequently exposed, whilst working at his trade, to the rays of a hot sun, he became affected with violent headache, dimness of vision, and eventually, in the space of eight days, lost his sight altogether. At his entrance into hospital blindness was complete, and there was considerable pain in the eyes. The pulse was slow but regular, and there was some stiffness of the neck. Energetic antiphlogistic and revulsive treatment, together with continued mercurial friction to the extent of producing profuse salivation, led, in fifteen days, to a very decided improvement in the vision; but no further improvement took place. On the contrary, symptoms of cerebral congestion ensued, and two months after the appearance of the disease the patient was seized with vertigo and contractions of the limbs. The pupils became fixed and dilated, and there was acute delirium.

The wife of the patient having informed the attending physician that her husband had had similar attacks before, which generally lasted several days, led him to the conclusion that the cause was, probably, of helminthic origin. He accordingly prescribed pomegranate rind, and three hours subsequently numerous fragments of tænia were expelled. The following morning the medicine was again administered, and a tænia, five metres in length, was passed. Amelioration of the general symptoms and of the sight at once took place, and the latter was ultimately completely restored.—*Recueil de Mém. de Méd., de Chir., et de Pharm.*

6. *Etiology of General Progressive Paralysis.*

According to M. Lagardelle, hereditary predisposition exercises a much less influence in causing general paralysis than in giving rise to mental alienation.

General paralysis is extremely rare before the 25th year, whilst insanity is very frequent before that age.

Women are more subject to insanity, and less disposed to general paralysis, than men.

Individuals of sanguine temperament, and of strong constitutions, seem to be more predisposed than others to inflammatory affections of the brain.

The professions which furnish the largest contingent to general paralysis, are those in the exercise of which a habit of venereal excess, and, above all, of the inordinate use of alcoholic liquors, is acquired.

The unmarried and those who belong to the higher classes of society are predisposed to contract this disease.

The antecedent diseases which predispose to general paralysis are those which are most often followed by mental alienation.

Onanism often produces imbecility and insanity, whilst in a large proportion of cases general paralysis is the consequence of venereal excesses.

Chronic alcoholism is a grave and frequent cause of general paralysis, whilst it exercises no influence in the production of insanity.

The suppression of some natural or artificial evacuation is rather a cause of insanity than of general paralysis. General paralysis appears to be less frequent in warm than in cold countries. The autumn and spring are the most fatal seasons for paralytics.

Moral causes predispose to general paralysis, whilst they do not cause mental alienation.—*France Médicale*.

The statement made in the last paragraph is certainly incorrect. Every physician knows the reverse to be the case. It is, probably, a typographical error.

7. *Clinical Remarks on Cases of Aphthæ of the Mouth and Throat, with Sickness and Diarrhœa, in Adults.*

Dr. Wilkes, on visiting a woman in his wards who was suffering from an aphthous condition of mouth and throat, with sickness and diarrhœa, stated that he had seen during the last few months several cases which were so much alike in their general features, that he considered them deserving of a distinct name—or, at least of a recognition which should place them in a special position by themselves. He was anxious to know whether such cases were common and had been met with by other medical men. The malady of which he spoke appeared one in which the whole alimentary canal was affected, as far as one could judge of the throat by the eye, and of the stomach and intestines by the sickness and diarrhœa. Dr. Wilkes has favored us with the following notes of some of his cases:

One of the earliest and most severe cases which I witnessed was that of a young man in consultation with Dr. Waterworth, of the New Kent road. I found the patient sitting up in bed dreadfully ill, making ineffectual attempts either to hawk up tenacious secretion from his throat or to swallow it. It was evident that his great trouble was in his throat; this was with difficulty examined, as its extreme irritability produced a constant dread of vomiting. The whole mucous membrane was of a deep red color, covered with white specks, patches and tenacious mucus; indeed, it was in a state which might be called follicular stomatitis, or even thrush—using the term in its most general sense. There was also considerable extension of the malady down the bronchial tubes, as evinced by the rales and the tenacious expectoration. He had already got much thinner, and his pulse was very rapid and feeble. He had constant sickness and diarrhœa. It should be said also that the secretion from the mouth was very fetid. If an examination of the throat had not shown an absence of false membrane, the case in other respects might have been regarded as one of diphtheria, judging from the extreme illness of the patient. Supporting remedies were evidently indicated, but they were taken with much trouble. He remained very ill for about two weeks, and then gradually recovered, but has scarcely regained his strength at the present time.

Another case was the daughter of a surgical instrument maker living at Lewisham. She was suddenly taken ill with severe febrile symptoms, and soon became affected with a very bad mouth and

throat, resembling the aphthous condition seen in children. She also had constant sickness and frequent diarrhœa; the skin was hot, and the pulse very quick and thready. After a few days the urgent symptoms passed off, but great prostration remained for a long time, and the disposition to sickness and diarrhœa continued for some weeks. She is now in a very precarious state.

A third case, somewhat resembling these, I saw with Dr. Butler, of Woolwich; the onset of the case was marked by delirium; but here the bronchitis was more severe, and the patient died in a very few days.

Another fatal case, with similar symptoms, I attended at Camberwell, with Mr. Lacey, but here the young man had been very dissipated, and had much mental depression, so that his previous condition may have had much to do with the result. The matter expectorated in this case was so offensive that the house was scarcely bearable, and suggested gangrene of the lungs, but of this there were no physical signs.

A lady at Croydon, after having a severe illness of a similar character to those mentioned above, suffered some time with stomatitis and bronchitis, and then gradually recovered.

I have within the last few days seen a sixth case at Rotherhithe for the second time. A young woman was taken suddenly ill with rigors, and in a few days was in an extremely critical condition. She had the same follicular stomatitis as in the other cases, the mucous membrane being of a deep red color, and covered with white spots and patches of secretion. It was so painful that she could scarcely swallow; at the same time there was very frequent vomiting and much diarrhœa. After being very depressed for some days, and giving her friends and the medical man much anxiety about the result, she slowly began to improve.

If one had witnessed but a single instance of this kind, the disease might have been attributed to some special exciting or individual causes; but when half a dozen cases of a malady come before the same practitioner and present the same symptoms, he can not but regard them as examples of a particular type of disease. I believe, therefore, that I am witnessing a malady which can not be referred to any one name in the category of disease. It has points of affinity with diphtheria; and, indeed, if the latter term can be used in a larger sense than that which implies the presence of a membrane, it is possible that very similar causes may be in operation for the production of the two diseases. I have used, as a conventional expression, the term "gastro-enterite," on the supposition that the whole mucous membrane of the alimentary canal was in a state of morbid action. This French expression I have never before adopted except in the case of children, in whom it is a convenient one; and, in speaking of children, I think I can not do better than explain the form of malady to which I wish to draw attention than by saying it almost exactly resembles that which medical men are in the habit of witnessing in infants, but transferred to adults. I refer to the very common cases of infants who, from error in diet or unknown causes, rapidly fall away in flesh, have vomiting, purging, and an aphthous state of mouth. In such cases, the nurse's opinion that the thrush has gone "through them" is not altogether erroneous, for I have seen the stomach and intestines show evident signs of morbid action, and the œsophagus covered with an adherent white secretion like that of the mouth, and corresponding very closely to a plate given in Cruveilhier's "Morbid Anatomy" under the head "Muguet."—*Medical Times & Gazette.*

EDITORIAL.

The cholera still persists. Its march has been slowly but surely forward towards the crowded cities of Central and Northern Europe. It has declined or entirely disappeared in the East, and in many places along the Mediterranean, to appear with equal virulence in others.

In Gibraltar the present epidemic is stated to be the most severe which has ever visited that place. The deaths had reached 380, out of a population of 24,000, in fifty-six days.

In France it is reported to have appeared at Nice, at Arles, Aix, Nîmes, Montpellier, Lyons and Paris. It is reported to be on the decline in Marseilles, while at Toulon it still rages with great severity.

It has also appeared, without doubt, in England. Several deaths from it have occurred in Southampton and neighboring towns, and in London three deaths have been referred to cholera, while diarrhœa marks the medical constitution of the season.

— Again it becomes our painful duty to record the decease of a worthy member of our profession. Dr. DAVID S. CONANT died Oct. 8th, at his residence, 27 East 24th street, after an illness of one week, at the age of 40 years.

Dr. Conant removed to this city from his native state, New Hampshire, fourteen years since, and at once became Demonstrator of Anatomy in the New York Medical College, which office he retained for six or seven years. On Prof. Peaslee's resignation of the Chair of Anatomy in the Medical School of Maine, in 1858, Dr. C. was elected Prof. of Anatomy as his successor. This position he retained till 1862, when Prof. T. Childs, who had succeeded Prof. Peaslee in that institution in the surgical chair, resigned; and since then, to the time of his death, Dr. C. had been Prof. of Surgery in the Maine Medical School. He had also been Prof. of Surgery in the Medical College at Burlington, Vermont, for the last five or six years. Besides, he had given instruction to a private class, in the various departments of medicine, regularly every season since he first came to this city, in addition to the two public courses of lectures just specified. His constant experience thus had rendered him a most accurate, thorough, and successful teacher.

He had also been one of the surgeons to the Demilt Dispensary for the past seven years, and, both from his varied experience there and elsewhere, and from an enthusiasm for surgery, as well as a native

quickness and dexterity, he had become one of our most accomplished and successful operators. Immediately after the battle of Antietam he volunteered his services for several weeks, and on that field he contracted a severe intestinal irritation from which he had never recovered.

Dr. Conant was connected with several of the medical societies of this city, and was ever ready to contribute his full share to their advancement. He had been President of the Pathological Society, and had held important offices in the County Medical Society, and the Academy of Medicine.

In addition to the labors already alluded to, Dr. Conant had acquired an extensive private practice, to which he gave the most faithful and conscientious attention.

None but the highest physical powers could endure the constant pressure thus incurred during the last five or six years; and even these were at last found insufficient, though he never seemed despondent nor declined to visit a fellow-being in distress. During the month of August last he was almost constantly occupied by night and by day in his practice, and was constantly exposed to septicæmic influences. He had had but little opportunity to recuperate in September, and on the last day of that month had a small furuncular inflammation on the right side of the nose. This was opened freely to the bone, and at once faded away. The next day but one, (Oct. 2,) inflammation recurred above the incision referred to, and was, in its turn, apparently subdued by a second free and deep incision. It returned a third time around and over the lacrymal sac, extended to the orbit, producing exophthalmia and a loss of sight of the right eye; and thence progressing backwards through the sphenoidal fissure, it attacked the membranes of the brain, and proved fatal on the 8th October.

In his relations to his professional brethren, Dr. C. was frank, cordial, genuine, and generous, and incapable of an unprofessional act. To his patients he was kind, faithful, and self-sacrificing to an extreme degree. As a citizen he yielded to none in public spirit and patriotism. But, more and better than all else, his was the life of an earnest and sincere Christian; and in death he was sustained by the religion which, from his youth, he had professed.

— Died, at his residence, Middletown, Conn., suddenly, September 26, 1865, Prof. CHANDLER R. GILMAN, M.D., of the College of Physicians and Surgeons of New York.

At a meeting of the Central Medical Association, held at Middletown, Wednesday, Sept. 27, the following resolutions were adopted:

Resolved, That by the recent sudden decease of Prof. CHANDLER R. GILMAN, M.D., of the College of Physicians and Surgeons of New York, our profession sustains an irreparable loss, a loss all the more keenly felt by us who, during the past two years since he took up his residence among us, have learned to love and honor him no less for those eminent professional and scientific attainments for which he was so long and widely distinguished, than for the cheerful, kindly, genial qualities of mind and heart which he ever displayed amid all his physical suffering.

Resolved, That we extend to his afflicted family and his fellow physicians in New York our warmest sympathy.

Resolved, That as a testimonial of our regard and respect for our deceased friend and his bereaved family, this Society attend the funeral services to be held Thursday, Sept. 28, at his late residence.

Resolved, That the Secretary be directed to furnish a copy of these resolutions to the family, and also to give them publicity.

A true copy.

GEO. W. BURKE,

Sec't Central Med. Association.

At a meeting of the Trustees of the College of Physicians and Surgeons of the City of New York, held on the 10th inst., the death of CHANDLER R. GILMAN, M.D., Professor of Obstetrics and the Diseases of Women and Children, having been announced, it was

Resolved, That the Trustees of this College have heard, with deep regret, of the decease of one of the most eminent of its Professors.

Resolved, That during the long period, during which the late Dr. GILMAN has taught in this College, the talent, earnestness and zeal with which he has devoted himself to the instruction of its pupils, has always secured him their entire respect and admiration; and the uniform urbane and considerate manner of his association with his class, has equally entitled him to their affection and good will.

Resolved, That these resolutions be published in the daily papers, and a copy transmitted to the family of the deceased.

GURDON BUCK, M.D.,
Registrar.

EDWARD DELAFIELD, M.D.,
President.

At a meeting of the President and Faculty of the College of Physicians and Surgeons, held Oct. 16, 1865, the following Preamble and Resolutions were unanimously adopted:

Whereas, it has pleased Almighty God to remove from this world our friend and colleague, Professor CHANDLER R. GILMAN, while humbly bowing to this dispensation of an All-Wise Providence, we, who have shared his toils and witnessed his devotion to the exalted duties of a self-sacrificing Profession, may be permitted to give some public expression to the feelings that so sad an event can not fail to excite; therefore,

Resolved, That in the death of Dr. GILMAN this Institution has lost an able, successful and distinguished teacher; its officers an intimate, tried and endeared personal friend; the medical profession a

wise and trusted counselor; and the community, a learned and deservedly eminent physician, a profound thinker, and a great and good man.

That, while from the Chair, which for twenty-five years he has adorned, he has eloquently taught the true principles of medical science and has distinguished himself as a leader in scientific progress, he has no less, in his blameless life, honorable character, genial sympathies, reverence for truth, respect for the rights of others, in his graceful literary attainments, in his faithfulness and zeal in all professional duties, and in his jealous watchfulness over professional purity and honor, left an example worthy the imitation of the thousands who have listened to his teaching and cherish his principles.

That, to his bereaved family we tender our heartfelt sympathy, invoking for them Heaven's choicest consolations; and assuring them that we shall ever cherish in our hearts pleasant memories of his life and labors.

J. C. DALTON, M. D.,
Secretary of the Faculty.

CHICAGO, ILL., Oct. 12th, 1865.

Editor New York Medical Journal:

In the report of the criticisms of my paper on the use of pressure in the treatment of gonorrhœal and purulent ophthalmia, published in the August number of your journal, I find the remarks made did not partake so much of an *analytical* turn as I had hoped.

The paper was a report of facts, abundantly witnessed and faithfully recorded.

I must regret that the gentlemen who did me the honor to criticise it wandered so far from the text.

If they will avail themselves of the means I have there recommended, experience induces me to believe that they will obtain more favorable results than would seem to have followed, in some cases, under the treatment advocated in their remarks.

Instead of a reserved and doubtful prognosis, with but few exceptions, these diseases can be satisfactorily controlled.

Truly yours,

JOS. S. HILDRETH, Surg. U. S. V.

— NEW YORK OBSTETRICAL SOCIETY.—The yearly election of officers for 1866 was held Oct. 17th, and the following gentlemen appointed: T. G. THOMAS, M.D., President; CHARLES HENSCHEL, M.D., Vice President; JOHN G. PERCY, M.D., Recording Secretary; ABRAHAM JACOBI, M.D., Corresponding Secretary; FESSENDEN N. OTIS, M.D., Treasurer.

NEW YORK MEDICAL JOURNAL,

A MONTHLY RECORD OF MEDICINE AND THE COLLATERAL SCIENCES.

DECEMBER, 1865.

ORIGINAL COMMUNICATIONS.

On Amputation at the Hip-Joint. By JOHN H. PACKARD, M. D., one of the Surgeons to the Episcopal Hospital, Philadelphia.

The remarks which I have to offer to the readers of the JOURNAL are based upon a case which occurred at the U. S. A. Hospital at Beverly, N. J., last winter. By the courtesy of Dr. Clinton Wagner, U. S. A., the surgeon in charge, under whose orders I was acting as Consulting Surgeon to the Hospital, the manual procedure was entrusted to me; but, as Dr. W. justly remarks in his (unpublished) Report of Surgical Operations, the successful result was mainly due to the surgical skill and the kind and unremitting attention of Dr. J. C. Morton, Executive Officer of the Hospital.

The patient was a private in the 11th Reg't Maine Volunteers, aged 19, and was brought to Beverly, August 22d, 1864, by steamer, having been wounded at Deep Bottom, Va., on the 16th. The ball had passed through the head of the tibia from before backward.

Sept. 12th, secondary hemorrhage having occurred, chloroform was given, and after a careful examination it was thought

proper to amputate, which operation was performed through the lower third of the thigh, by the circular method. No untoward symptom was observed until October 17th, when bleeding was again set up, and the femoral artery was cut down upon and tied in Scarpa's space. The ligature came away in ten days.

November 5th, the end of the femur protruding through the retracted soft parts, about four inches of the bone were removed by means of the chain-saw. Shortly after this, the stump became enormously swollen and painful, and abscesses formed here and there in it.

January 19th, 1865, the bone was exposed, and found to be greatly enlarged, and in a state of necrosis, as high up as the trochanters. The patient being already under chloroform, the femoral artery was at once exposed and tied in the groin, and the disarticulation of the hip performed by antero-posterior flaps. Some slight difficulty was experienced in controlling the artery accompanying the sciatic nerve, but the quantity of blood lost in the operation was not large. Extreme depression was exhibited, the patient being of necessity kept on the amputating table for two or three days, lest the effort at removal should prove fatal. Large quantities of stimulants and concentrated food were administered, and the surface temperature artificially maintained.

January 27th, bleeding again occurred, and the external iliac artery was tied. The ligature came away in twenty-one days.

February 19th, two days afterwards, the lower end of the divided artery poured fourth blood furiously, and was only controlled with great difficulty by direct pressure, which was kept up for about two weeks.

After this, recovery progressed steadily, and by the end of March the man was well. In May, on the breaking up of the Beverly Hospital, he was transferred to that at Whitehall, and in June he was sent to his home in Maine.

Probably most of the readers of the JOURNAL are acquainted with the case lately published by Dr. Van Buren, of New York, in his valuable "Contributions to Practical Surgery." Here the patient's condition was much more favorable. The first

operation was done for disease of the femur, of twenty years' standing; the second for return of the disease, about two years after. No untoward accident in the shape of hemorrhage occurred. Death took place five years later, from renewal of the disease in the pelvic bones.

Dr. Van Buren refers to three other cases in which amputation at the hip was successfully done after previous removal of the same limb above the knee: one by Sir A. Cooper in 1824, one by Mr. Mayo in 1841, and one by Mr. Sands Cox in 1844.

Another, making six, has been published by Fayrer, of Calcutta. I met with the account in a recent number of the *British Medical Journal*; the exact reference has escaped me. It is so interesting that an apology is hardly necessary for reproducing it here. "The operation was performed when the patient was very low, suffering from clear indications of blood contamination, the result of a diseased condition of the medulla, which is unfortunately frequent in India after section of the long bones, and the cause of many unsuccessful amputations. The operation was performed and the recovery occurred at a very hot season of the year, the thermometer ranging from 86° to 104° . Cholera and other diseases were very prevalent at the time.

"On April 10th, 1864, a native boy, 16 years old, was thrown from a horse: at the inner side of his knee the soft parts were severely injured, but the joint was, apparently, unhurt; on the 12th it was found that the joint was opened. The limb was then removed at the lower part of the thigh. After the amputation, fever and extensive necrosis of the bone followed, so that, as a chance of saving life, the limb was removed at the hip-joint. The knife was entered a little above and in front of the great trochanter, and emerged at the root of the scrotum. The flap being raised, the femoral artery was tied before the posterior flap was cut. On dividing the bone at the great trochanter drops of pus oozed out of its cancellated tissue; Dr. Fayrer thereupon seized it with the lion-forceps and dissected it out without loss of time. The acetabulum was healthy. All bleeding points, venous and arterial, were tied. The loss of blood was very small—less than eight ounces. His pulse, which was over 150 when the operation was commenced, was

very little weaker after it was over. Stimulants were given and hot bottles applied. After the operation the patient immediately improved, and eventually recovered. The last report of him is as follows: 'He goes to work regularly as a tailor, and is in robust health. He uses crutches and gets over the ground rapidly; is getting fat, and is much grown in height as well as circumference since his accident. He was admitted on April 10th, 1864; thigh amputated on April 12th; hip amputated on April 24th; perfectly cured on July 13th, 1864—just one hundred days from the operation.'

Dr. Gross, in speaking of this subject, says: "Of seven cases of this kind, in the hands of Astley Cooper, Textor, Mayo, Cox, Syme, Bradbury and Van Buren, all were successful. In an instance in the practice of Mr. Guthrie, where the operation was performed on account of gangrene and hemorrhage, after amputation of the thigh for a gun-shot wound, the result was fatal." *

Four of these cases, those, namely, of Textor, Syme, Bradbury, and Guthrie, I know of through this quotation only; but they bring the whole number of cases up to *ten*, with only one fatal issue. From them it seems to me that some valuable hints may be gained. In two of them, my own and Fayrer's, the circumstances were most unpromising; in the former, the great natural courage of the patient, and the untiring assiduity with which he was watched over by Dr. Morton, carried him through the fearful experience which has been detailed. I know of no parallel case on record.† Disarticulation of the

* System of Surgery, vol. ii., p. 1046. (3d edition.)

† I am tempted to quote here a curious passage, which I met with a few days since in a lecture delivered by Mr. (afterwards Sir) Charles Bell, at the school in Great Windmill street, London, in 1824. He says:

"The mania for amputation at the hip-joint, which has of late years prevailed, I have seen finely exhibited in an individual, who, when the subject was mentioned, actually tore his hair, and exhibited the appearance of the deepest distress; one might have supposed that some of his dearest friends had fallen sacrifices to this operation; but no, the feeling was excited by his recollections being awakened, by the sight of a carious thigh bone, of an opportunity of operating which he had lost."

In a foot-note Mr. Bell adds:

"A friend, on reading this, reminded me that he had been present at this singular exhibition of professional zeal, and states, what I had forgotten, that

hip is, both to the patient and to the operator, one of the most formidable procedures in surgery, whether we look upon it in its immediate surroundings or in the light of its statistics. The reason does not, however, clearly appear why so many of these cases should turn out badly. Sédillot says: "Its dangers are due to the proximity to the trunk, the extent of the wound, the mass of flesh divided, the difficulty of obtaining union, and the nervous shock arising from the loss of a member representing nearly one-fourth of the entire mass of the body; which shock is so great that the patients often fall into a complete collapse, and die without any assignable cause."*

On the other hand, Erichsen says: "In amputation at the hip-joint the great danger to be apprehended is excessive hemorrhage, the incisions being made so high up that no tourniquet can be applied, nor pressure of the groin trusted to."†

Dr. Gross says that the great risk which attends this operation "is due to the loss of blood, suppuration, erysipelas, and pyæmia."‡

It would take up too much space to adduce other opinions; those quoted embrace the views of leading writers of the present day in this country, England and France. Some of the sources of danger mentioned may be set aside, as not especially belonging to amputation at the hip.

Pyæmia, erysipelas and excessive suppuration may ensue upon much slighter operations. Hemorrhage may be altogether prevented by compressing the abdominal aorta, either by means of a large clamp tourniquet or by the fingers of assistants.

Ovariectomy, herniotomy, the Cæsarean section, all these show a larger proportion of successful results, and yet they

the enthusiast, in alluding to the particular instance in which he might have performed the operation, told us 'that as the child had previously lost the greater part of the limb by amputation for disease of the lower part of thigh-bone, there would have been little danger from the shock of separating such a mass as the quarter of the body; I should have only,' said he, 'had to pick out part of the bone from the socket; and thus I should probably not only have been the first of the few whose patients have survived this operation, but have been even the first to have performed it.'"

* *Traité de Médecine Opératoire*, etc., tome i., p. 157. (Paris, 1853.)

† *Science and Art of Surgery*, p. 48. (London, 1861.)

‡ *Op. cit.*, p. 1043.

would at first sight seem to involve even graver risk than the disarticulation of the hip. Against the exposure of the large wound-surface in the latter, we have to set off the opening of the peritoneal cavity, so often necessary even to a wide extent in the other operations mentioned.

Probably the true cause of the mortality in coxo-femoral amputation is to be found in the great mass of living tissue removed, and the shock thereby involved; an idea which is supported not only by the fact that the statistics of amputation of the thigh in its upper third are nearly as unfavorable, but also by the far better results attending the operation when the previous removal of the thigh has done away with the circumstance alluded to.

If now we look into the subject of the ordinary operation of amputation at the hip-joint, we shall find in the first place that the greater proportion of successful cases have been those of disease; and that the patient's chances of benefit are increased in traumatic cases by delaying the operative interference as long as possible.*

To quote the experience of American surgeons only, I have been able to collect eight cases of successful amputation at the hip-joint, but one of which was for injury. This one was done by Dr. Edward Shippen, of this city, while in the army; it was performed for a gun-shot wound of the femur, received six hours previously. The patient was subjected, a month afterwards, to the horrors of a Richmond prison; and yet his recovery was perfect.

Mott operated for disease following a badly united fracture; Duffee for coxalgia; Gross for deformity after a burn; Pancoast once for osteo-sarcoma, and once for some other disease to me unknown; Warren for osteo-sarcoma; May for caries of the upper part of the femur.†

* Gross, op. cit., p. 1046. Legouest, quoted in "Longmore on Gun-shot Wounds," p. 115. (The principle as laid down by Legouest was confirmed by a Committee of the *Société de Chirurgie* of Paris, in 1860.)

Baudens puts this very forcibly: "Let us remember that, while the disarticulation of the knee should be done at once, that of the hip seems not to succeed (*paraît ne pouvoir réussir*) unless delayed some time after the receipt of the wound." *La Guerre de Crimée*, p. 132.

† I have been told, but am inclined to doubt the story, that amputation at

Contrasting this list with those so much more familiar, (for instance, Legouest's, of thirty primary operations, all ending fatally,) we can not but regard the prognosis in cases of disease as far more favorable than in cases of injury. The opposite opinion prevailed until set aside by experience. Dr. Pancoast, in his "Operative Surgery," published in 1844, says: "It may be important, however, to observe that nearly all the successful cases have been those in which the operation was practiced for traumatic injuries, and almost immediately after their infliction; while the greater number of fatal results have been consequent to the operation on subjects previously exhausted to more or less extent by disease." I have no doubt that this eminent surgeon would alter this statement were he to write at present on the subject, and mention his view, as expressed, in order to show by how high authority it was indorsed.

If, then, we consider the cases in which the surgeon may be called upon to undertake the coxo-femoral disarticulation, we find them divisible into four classes, according to the degree of probability of success.

(1.) Those in which the same thigh has been previously amputated for injury or disease.

(2.) Those of chronic disease. It would scarcely be fair to place cases of hip-joint disease in this class, although the first successful case in this city (Philadelphia) was of this character. It so often happens that the acetabulum is seriously involved, that in many cases no operation could be of benefit.

(3.) Those in which an attempt has been made to save the limb after injury, and this operation becomes the only hope of the patient.

(4.) Those in which the desperate character of an injury recently inflicted renders death inevitable, unless this slender chance is afforded.

Even in the most favorable cases of the first of the above mentioned classes, amputation at the hip-joint is not to be lightly undertaken. I do not even consider it, as asserted by some writers, one of the easier amputations to perform. The necessity should be stringent, the weighing of the chances careful, the

the hip-joint was twice performed with success by rebel surgeons, during the late war, for gun-shot injuries.

decision conscientiously arrived at. But it does seem to me that the degree of success attained in the recorded cases is such as to make it the imperative duty of the surgeon to perform the operation under the circumstances indicated. In other words, it is not a matter of choice for him whether he will seek to exhibit his prowess with the knife or avoid the risk of failure. He is not only justifiable in operating, but he would be unjustifiable in not doing so.

Should the result be unfavorable, he may, it is true, have painful doubts as to the propriety of the course he was led according to his best judgment to adopt. Probably all honest and conscientious surgeons have known what it is to be so troubled—some, from their mental peculiarities, more than others. And such doubts would be more likely to arise when operative interference had been resorted to than when it had been decided against. Still, this is one of the elements of the responsibility assumed by the surgeon, and can not be evaded.

Before concluding these remarks, it may be proper to observe that, in regard to all operations, a larger proportion of the successes are apt to be placed on record than of the failures. And such may be the case with the amputations at the hip after previous removal of the same thigh at a lower point. But when we consider the very extensive discussion of the general subject of coxo-femoral disarticulation, and the fact that an operation of such magnitude is not apt to be confined to the knowledge of a few persons only, we may fairly suppose that the known cases of the kind just spoken of afford at least as correct a basis for the estimate of a patient's chances as we have for our guidance in regard to any other surgical procedure.

1415 Spruce Street, Phila., Oct., 1865.

On the Treatment of a Certain Form of Paralysis occurring in Children. By WILLIAM A. HAMMOND, M.D., of New York.

The disease, the treatment of which I propose to consider in this memoir, is not the temporary paralysis of infancy which

several years ago was so well described by Kennedy, (*Dublin Medical Press*, September 29, 1841. and *Dublin Quarterly Journal of Medicine*, February, 1856.) but that of which Duchenne has treated, (*De l'Electrisation Localisée.*) as consisting essentially of fatty atrophy of the muscles. I shall not at present dwell upon the many points connected with the pathology of the affection in question, mainly for the reason that I have no very definite ideas upon the subject. I am, however, disposed to regard it as a disease in which the muscles are atrophied, and their irritability impaired or altogether destroyed, without any necessary conversion of their tissue into fat. The views which have hitherto been expressed upon this subject are in the main based upon conjectures, and I do not claim for my own any more solid foundation.

CASE I.—H. J., male, aged five years, came under my care April 19th, 1865, to be treated for paralysis of both lower extremities. During the previous summer the child had suffered from hooping-cough, and when the disease was at its height motion and sensation were suddenly lost in both legs, from the hips down. Medical advice was at once obtained, and various measures were in consequence adopted, without any material benefit. Sea-bathing was then recommended, and this was faithfully persisted in for several months, with the result of restoring sensibility to both limbs, and motion to the muscles of the thighs. Since then strychnia had been administered, both by the stomach and by subcutaneous injections, without the least improvement being effected. Upon examination with the æsthesiometer I found the sensibility of both limbs tolerably good. The mercury of a delicate thermometer, the bulb of which was applied to the thigh, stood at 90°, whilst below the knees the temperature was but 82°. The child was able to flex, extend, rotate, abduct and adduct the thighs, and to flex and extend the legs. There was no power, however, over the feet, and upon careful examination I could not find that a single muscle situated below the knees was capable of contracting from strong induction currents. Both legs were atrophied. They were of the same size, being at the largest part six and a quarter inches in circumference.

Aside from the paralysis the child appeared to be in good

health. Its appetite was good, there was no pain, and it slept well at night.

I directed that night and morning both legs should be put up to the knees in water of the temperature of 110° , and kept there for twenty minutes; that they should then be well rubbed for half an hour with a coarse towel, and the muscles kneaded for the same period; the child was also to be brought to me three times a week for faradisation.

This treatment was continued for three weeks with but little if any benefit. During this time I had continued to use very strong induction currents for fifteen minutes to each leg three times a week. The machine, which was very powerful, was put in action by a battery consisting of three Smee's cells. The current excited caused the most intense pain, but did not produce the slightest apparent contraction in any muscle. I then determined to make use of the constant current derived from a voltaic pile of one hundred pairs—and consequently possessed of great intensity. The poles were applied first to the tibialis anticus of the right leg. The instant the circuit was made the foot moved up. By continuing the experiment, I found that contractions could be induced in every muscle of both legs. I then had an arrangement constructed for making and breaking the circuit rapidly, and persevered with the treatment daily for a week. During the whole of this period, at every trial contractions were invariably induced in every muscle upon the circuit being made and broken. The warm water frictions and kneading were also continued. I now found that the temperature of the legs below the knees was 86° , and that the circumference was, at the former place of measurement, seven and one-eighth inches. The facts that the toes could now be slightly flexed and extended by voluntary efforts, and that there was some little power over the gastrocnemii muscles, assured me that the cure would ultimately be complete. In this hope I was not disappointed. Amendment continued, and on the 17th of August, when I saw the child for the last time professionally, power over all the muscles of both legs was almost completely restored. Very feeble induction currents now caused contraction. The tibialis anticus was still, however, weak; but I have no doubt that by exercise it,

as well as all the rest, will become well nourished and strong. At this date the circumference of the legs was eight and a half inches, and the temperature 90°.

CASE II.—M. W., female, aged three years, was brought to me Dec. 6th, 1864, suffering under paralysis of the right lower extremity, the consequence of a fever with which she had been affected the previous summer. Upon examination, I found the temperature of the leg below the knee six degrees lower than that of the other limb. The circumference at the fullest part of the calf was an inch less; sensibility was obtuse, though not entirely abolished. With the exception of the flexor brevis digitorum, there was complete paralysis of all the muscles which act upon the foot and toes. There was not the slightest contraction produced in any other by strong induction currents.

Previous to my seeing the child, faradisation had been imperfectly used, and strychnia and stimulating liniments had been employed without any good effect. The opinion was expressed by several eminent physicians that a cure was impossible.

I determined to make use of very powerful induction currents, hot water, rubbing and kneading, as in the case described. I continued these measures, and by the 27th there was very considerable amendment. Faradisation had been employed at intervals of two or three days throughout the interval. The temperature of the leg had increased, and contractions of the extensor muscles of the foot and toes could be excited to a slight extent. There was no increase of voluntary power.

On the 20th of January I applied a battery to the limb, consisting of a plate of zinc and one of silver, connected by an insulated wire. The zinc plate was kept in contact with the thigh, whilst the silver plate was placed on the anterior part of the leg. The arrangement was worn constantly for several weeks, whilst the other measures were not discontinued. By the first of March there was a very decided improvement manifested in all the symptoms, and there was an undoubted increase of voluntary power. Still the contractions caused by the induced current were very feeble, and in some of the muscles, as the tibialis anticus and peronei, could not be ex-

cited at all. I therefore determined to make use of a more powerful continued current, and had the battery constructed which has been referred to in the history of the previous case. As soon as the poles were applied to the skin over the tibialis anticus, this muscle, and others in contact with it, contracted powerfully. The peronei also acted well under its influence. I continued to make and break the circuit over different points of the leg for fifteen minutes, every time causing strong muscular contractions. The treatment was carried on three times a week till the 1st of June, at which time voluntary power was restored to every muscle of the leg and foot. The tibialis anticus and peronei were still feeble, but, with all the others, had become responsive to induced currents. During the months of June, July and August the child was sent to the coast and sea-bathing was used every day. During this period no electricity was employed. It was resumed again on the first of September. At the present date (October 20th,) the little patient is almost well. The posterior muscles of the leg and those on the side of the foot are perfectly restored; the extensors of the toes are also quite powerful, and the peronei act well; the tibialis anticus is the only one which is not entirely subject to the action of the will. The temperature of the leg is not appreciably below the other; it has not, however, regained its full size, though it is gradually improving in this respect. The lameness, which at first was very well marked, is now scarcely perceptible, and is entirely obviated by a brace which prevents her dropping her shoulder—a habit she has acquired through the limbs being weak. Galvanism and faradisation are still continued once a week to the tibialis anticus.

In this case, I am very confident that, but for the persistent use of galvanism and faradisation, the child would never have recovered from the paralysis.

CASE III.—W. S., male, aged four years, was placed under my care September 2d, 1865, with complete paralysis of the left deltoid muscle, which had persisted for over a year, and which had ensued upon an attack of measles, attended with great pain in the back. Originally the whole extremity had been paralyzed, but the other muscles recovered their contractile power in a few days. At the time I saw this child they

all responded actively to induced currents except the deltoid, which was absolutely devoid of all irritability. The arm could not, therefore, be raised from the side. The muscle was shrunk-en, and the shoulder, in consequence, much flattened.

As I have said, induced currents failed to produce the slightest action in the muscle, and though I applied the full power of an induction apparatus of much greater strength than Duchenne's or Rhumkorf's, or any other I have ever seen used in medicine, no perceptible result followed. Upon applying the direct current of my voltaic pile, a strong contraction ensued, and similar actions followed on each formation and rupture of the circuit. This treatment was continued three times a week till the 24th. At this time slight movements could be accomplished by the exercise of the will. Induced currents were now used with the effect of causing strong contractions. Amendment continued to take place, and by the 10th of October, the muscle had acquired almost its full power. The child could raise the arm from the side with ease, and hold it in this position for half a minute. The atrophy had also nearly disappeared. The treatment was now discontinued, and gymnastic exercises recommended.

I have selected the foregoing cases from several others, as presenting a fair idea of the action of the continuous galvanic current of great intensity in exciting muscular irritability when it has been apparently altogether lost, so far as other means enable us to determine. After contraction has been well established, and the will begins to assume its power over the affected muscles, I prefer to use the induced or faradaic currents, as being more local in their effects. The continuous current, as I propose to show in a subsequent memoir, does not limit its action to the part through which the galvanism passes, but affects distant regions of the body.

The voltaic pile of which I make use is one which I devised myself, and which I find to possess great intensity. It is constructed of perforated zinc and copper gauze cut into square pieces soldered together, and the couples separated by pieces of woollen cloth. It is set in action by strong vinegar, a few seconds' contact of the poles (terminated by wet sponges) with the skin will cause vesication. Its use, therefore, requires

caution. It can not be applied to the face, or any part of the head and neck to which the fifth pair of nerves is distributed, without risk of causing great disturbance of vision and perhaps blindness from over-excitation of the retina.

Report of a Case of Gun-shot Injury of the Median and Internal Cutaneous Nerves of the right upper extremity; Amputation, after unsuccessful Treatment by Subcutaneous Injections of Morphia; the Dissection of the Nerves from the surrounding Cicatrix, as done by Warren; and Excision of a portion of the Nerves. Recovery. By GEORGE A. MURSICK, M.D., of New York, late Asst.-Surg. U. S. Vols.

Private John C. Marks, Co. D., 149th Pa. Volunteers, aged 28 years, and of a nervous temperament, was admitted to the Stanton U. S. A. General Hospital, Washington, D. C., May 13th, 1865. He had been wounded May 10th, in the Battle of the Wilderness, Va., by a musket-ball, while in the act of ramming a charge into his own musket. The bullet entered the left arm above the external condyle of the humerus, and passed obliquely through the muscles, anterior to the bone. It then entered the inner side of the right arm, in the middle third, and passed through it, emerging at about the junction of the middle with the upper third, antero-posteriorly. When admitted he was in good condition, and suffered from no constitutional disturbance. He complained, however, of a slight tingling sensation in the fingers of the right hand, supplied by the median nerve. Water dressings were applied to the wounds, and they healed in about six weeks. During this time the tingling sensation in the fingers had increased to decided pain. As the consolidation of the cicatrix went on the pain increased, and in a short time became almost unbearable. He could get no rest, and suffered great constitutional irritation. The temperature and sensibility of the forearm and hand became exalted; the skin was of a purplish-red color, the fingers stiff and extended. Nutrition became impaired, and he was slightly emaciated. The pain in the fingers and hand was increased by pressure upon the cicatrix of the wound.

June 20th. Various applications have been made to the limb with but little relief to the pain. To have morphiae sulphat. gr. ss. in solution injected under the skin of the arm, near the cicatrix, and to take ferri et quin. citrat. gr. v., three times a day. The injection of morphia gave decided relief to the pain, so long as its influence continued, which, in the beginning, was about eighteen hours, when the pain became as severe as before the injection. As the time of its influence diminished, it became necessary to increase the frequency of the injections.

July 13th. R. Morphiae sulphat. gr. ss. in solution by subcutaneous injection, morning and evening. Hand and forearm to be painted with tinct. rad. aconit. twice daily.

August 20th. Treatment continued. He continues in about the same condition, except that he complains of some stiffness of the jaws. The morphia and other treatment has afforded no permanent relief to the pain.

August 23d. The stiffness of the jaws (trismus) has increased. He can only open them about half an inch, and can take no solid food. Exhibits the risus sardonicus in a slight degree. R. Subcutaneous injections of morphia to be increased to gr. ij. daily. Under the increased quantity of morphia the trismus gradually disappeared, and did not return until the 16th of September.

Sept. 16th. The treatment thus far has proved to be of no avail, and freedom from pain only continues as long as the effects of the morphia continue. It was now decided to perform an operation for his relief, which was done by Surgeon J. A. Lidell, U. S. Vols. After being etherized, an incision of about two inches in length was made through the cicatrix down to the median nerve, which was carefully dissected out; the internal cutaneous nerve was also found to be involved in the cicatrix; this was also carefully dissected out, together with all the cicatrixial tissue which surrounded both of them. There was no apparent injury to the nerve other than the constriction of it by the cicatrix. The neurolema of them was perhaps a little redder than natural. The edges of the wound were brought together with adhesive straps, and cold water applied. During the exhibition of the ether severe spasms of

the laryngeal muscles occurred, and he became partially asphyxiated. The ether was suspended, ammonia applied to the nostrils, and the spasms ceased, and he soon breathed free again.

Sept. 17th. Has passed a tolerably comfortable night without morphia. Says he feels very drowsy this morning. The pain in the hand has not entirely ceased. Bowels confined. R. Sal Rochelle, \mathfrak{z} i. P. M.: The pain in the hand is very severe, almost as bad as before the operation. R. Morph. sulph. gr. i. by subcutaneous injection.

Sept. 18th. Says he has felt very comfortable since the injection of the morphia. Had a good night's rest. He exhibits considerable nervous irritability, and has some stiffness of the jaws. R. Zinci. sulph. gr. i. every six hours.

Sept. 19th. The trismus has entirely disappeared. Can open his jaws to the full extent. Towards evening the pain in the hand increased so much that morphia was required to give relief from pain.

Oct. 1st. The pain in the hand continues as before, but is more severe at night. The subcutaneous injections of morphia have been continued, (gr. i. daily.) The severity of the pain has not decreased much since the operation. The wound has nearly healed.

Oct. 6th. The wound has entirely healed. The pain is as severe as before the operation, and at times he suffers from severe attacks of tetanic irritation. This morning he suffered from a severe attack. The previous operation having failed, and the treatment affording only temporary relief, his general condition being much worse from the severe nervous irritation and loss of rest from which he was suffering, it was decided to perform excision of a portion of the nerve, and to save the limb if possible—as it is well known that the nervous tissue will be regenerated after a time and the functions of the parts restored.

I made an incision through the old cicatrix, and carefully dissected out the nerves, (the median and internal cutaneous.) The anatomical relations of them had been disturbed by the previous operation; both were found lying side by side, completely surrounded by the cicatritial tissue, which was very

vascular. The sheaths of the nerves were very red in color, otherwise they presented no abnormal appearance to the eye. I divided them, and resected the portion which presented the abnormal color, (about $\frac{3}{4}$ of an inch.) The edges of the wound were approximated and retained by adhesive straps, and a bandage applied to the whole arm.

Oct. 7th, 2 o'clock, P.M. Has been almost free from pain until the present time, and passed a very comfortable night. He now complains of a severe pain in the hand and forearm, as severe as at any time before. His skin is cool; he is sweating profusely—large drops of perspiration stand out upon his forehead and face. Pulse frequent (120) and soft. R. Morphine sulph., gr. i., by subcutaneous injections.

He states that since the nerve was divided he feels "a pricking sensation" in the fingers, to which it is distributed. He thinks that he can feel when pressure is made upon the fingers, though the sense of touch is lost, and the hand feels "heavy and dead."

Oct. 19th. The wound of operation is nearly healed. The pain is now as severe as before the nerve was divided, and is very acute and nearly constant; it is situated in the same parts as before, viz.: the palm of the hand, the thumb, index and middle fingers. The treatment by subcutaneous injections has been continued, from one to three grains having been injected daily. No treatment appears to avail any thing, and there appears to be great danger of the occurrence of tetanus. He expresses a strong desire to have the arm amputated to relieve him from the pain. Surgeon J. A. Lidell, U. S. Vols., amputated the arm, by the double-flap operation, at the junction of the middle and upper third, the patient being under the influence of chloroform. The stump was very vascular and required a large number of ligatures, and the free application of the liquor ferri persulphat. to control the general oozing from it. The flaps were not coaptated. The stump was dressed with charpie and a roller bandage applied up to the shoulder. 9 o'clock, P.M. He complains of great pain in the stump; he says that it feels as if something was "twisting the nerves;" is very restless and exhibits great nervous irritation. R. Morphine sulph. gr. i.

Oct. 20th. Has passed a very restless night; the stump is excessively painful and is slightly swollen. Tongue, large, pale and flabby, is indented on the edges by the teeth; pulse frequent and irritable. *R.* Morphiæ sulph. gr. i. by subcutaneous injection; ice dressing to stump; spts. vini gallici ʒi . every six hours.

Oct. 21st. Dressed the stump; has suffered a great deal of pain during the past twenty-four hours; the swelling of the stump has increased; tongue furred; bowels confined; pulse 120 and feeble; has severe spasms of the muscles of the stump, and some stiffness of the jaws. *R.* Pulv. opii gr. i., ext. belladonnæ gr. $\frac{1}{2}$ in pil. every two hours; lead and opium lotion to stump; brandy ʒiv . every two hours.

Oct. 22d. Has passed a very quiet night; the pain in and the swelling of the stump is much less; pulse 98 and fuller; bowels have not moved. *R.* Sal. Rochelle ʒi .; continued other treatment. P. M., 5 o'clock, has vomited the salts which he took this morning; stomach very irritable: *R.* Sinapism to epigastrium, and an enema to move the bowels.

Oct. 23d. The enema moved the bowels moderately; nausea continues; has vomited several times during the night; the stump is suppurating. *R.* Hyd. sub. mur. gr. v., pulv. rhei gr. x., sodæ bicarb. gr. x., ft. pulv.; to take at once. P. M., 6 o'clock, bowels have moved freely; is very restless and irritable; stump painful. He says the pain occurs in paroxysms, and is very "sharp and cutting." *R.* Pulv. opii gr. i., ext. belladonnæ gr. $\frac{1}{2}$ every three hours.

Oct. 24th. Has passed a quiet night and slept tolerably well; discharge from the stump is quite free, it has nearly cleaned off, and florid granulations are springing up; his appetite is improving; pulse 90 and moderately full. 12 o'clock, A.M. The nurse called me and stated that the patient was out of his head; I found him in a very excited condition, exhibiting great nervous irritability. He answered questions rationally when spoken to, but his mind wandered from the subject. He appears much depressed in spirits, and expresses gloomy forebodings of the future. Skin cool and moist; pulse 110 and feeble. *R.* Sinapism to back of neck, Hoffman's anodyne ʒss .,

fluid ext. valerian 3ss. every hour; spts. vini gallici 3iv. every two hours.

6 o'clock, P.M. Pulse 110 and fuller; skin warm and moist; is much less restless and his conversation is coherent and rational. R. Magendies solut. gtt. xv. at bed-time.

Oct. 25th A.M. Has passed a quiet night and slept considerable—expresses himself as feeling much better. His bowels moved freely during the night. He still exhibits a good deal of nervous irritability and watchfulness, but the depression of spirits is much less; pulse 120; R. Fluid Ext. rad. aconit. gtts i. every two hours and milk-punch freely. 6 o'clock, P.M. Pulse 110; has passed a quiet day, but has not slept. R. Magendies solut. gtt. xv. at bed-time.

Oct. 26th. Has had three or four hours of sound sleep during the night and feels much refreshed; pulse 100, moderately full but irritable. All the ligatures came from the stump this morning except the one on the brachial artery. The face of the stump looks well, but it is still swollen, and the muscles protrude beyond the skin and are very irritable, so much so that when the stump is dressed there is a constant twitching of them which continues for an hour or more. Bowels confined. R. Pil. cathart. co. No. 2.

6 o'clock, P.M. Says he feels much better to-night, and as if he had awakened from a dream. His bowels have moved freely; appetite improved, and he expresses a desire for food. Continue morphia at bed time.

Oct. 28th. He continues to improve, the stump suppurates freely; the swelling and irritability of the muscles is much less; pulse 100 and of good volume. He says that the "dreamy sensation" has passed away and that he feels like himself again. The ligature came from the brachial artery to-day; bowels confined. R. Pil. cathart. co. No. 2.

From this time he continued to do well. All the nervous phenomena subsided; the stump healed slowly; he gained in flesh and spirits, and was finally discharged from the hospital in good condition.

The autopsy of the amputated limb revealed no appreciable abnormal condition of the nerves. Nothing was dis-

covered that could account for the peculiar nervous phenomena exhibited.

The limb was abnormally vascular, and the muscles exhibited fatty degeneration in a slight degree.

About one hundred and forty-five grains of the sulphate of morphia were administered by subcutaneous injections in various parts of the body, without affording any permanent relief. At first they were confined to the arm above the wound until they gave rise to induration of the cellular tissue, then to the arm below the wound and in the forearm; in the opposite extremity, in both lower extremities, and in the abdominal walls. No difference was found in the effect produced by changing the locality of the injections. Relief from the pain followed in from twenty to thirty minutes subsequent to each injection, and the trismus and other tetanic symptoms were controlled, temporarily, by increasing the quantity of morphia from time to time as occasion required.

On the Propriety of Inducing Premature Delivery. By T. G. THOMAS, M.D., Professor of Obstetrics and the Diseases of Women and Children, in the College of Physicians and Surgeons, New York.

[Read before the N. Y. Obstetrical Society.]

On the 1st of June, 1865, Mrs. B., a short but well made woman, aged thirty-three, applied to me to take charge of her delivery, which was anticipated about the 5th of September, giving me the following history. She had been delivered ten months before of a still-born child at the seventh month, labor being ushered in by a violent convulsion which destroyed the life of the child, and left the mother in so precarious a condition that her life for several days was despaired of. During her illness she was attended by an intelligent practitioner of Saratoga, and Dr. Willard Parker, of New York, in consultation. After recovery from this confinement, she was amaurotic for three months, and was during that time supposed to be

suffering from Bright's Disease. Becoming again pregnant, she naturally dreaded a recurrence of convulsions, and fears of a fatal issue for herself and offspring filled her mind with anxious forebodings—more especially as one of her sisters, an unmarried lady, had since that time died of Bright's Disease.

Upon taking charge of the case, I made a careful chemical and microscopical examination of the urine, and found it free from evidences of disease. These examinations were repeated weekly with the same result; and as I left the city on the 1st of July, to be absent a month, I left my patient under the care of Dr. Charles A. Budd, who watched the secretion as I had done, and reported her doing well on the 1st of August. On the 12th of that month, I was called hastily to Mrs. B., and found her laboring under some dyspnœa, considerable confusion of intellect, dizziness of head and nausea. Edema of the feet, hands and neck was discovered, and upon examining the urine, which was scanty and high colored, I found it loaded with albumen. After this I saw her daily, and adopted those measures most relied on in such cases as preventive of the threatening climax. From day to day the symptoms above recorded steadily increased, the œdema of the neck becoming so marked as to be oppressive, and great depression of spirits superadding itself to the array of morbid signs. On the 16th, Dr. Thebaud saw her with me in consultation, agreed with me in the propriety of inducing premature delivery for prevention of convulsions, which, from the peculiarly marked features of the case, we regarded as more than probable; and in accordance with the result of the consultation, I proceeded to accomplish it on the 17th.

On this date, at 1 P.M., I introduced into the cervix a large sponge tent, but finding it impossible to retain it in position I removed it and used the smallest of Barne's dilators. After twenty-five minutes I had dilated the os to the size of a circle whose diameter is two inches. I now left her, and returned to her at 4 P.M. No labor pains having been excited, I introduced the largest dilator, and after dilating to its utmost capacity, introduced a No. 10 gum-clastic catheter between the membranes and uterine wall, and left it. At 7 P.M. I was sent for in haste, violent expulsive pains being

established. Upon my arrival the desired process was found fully inaugurated; the lady, as a further preventive means, was put under chloroform, and at 11½ P.M. a female child was born.

Mother and child did well, no untoward symptom showing itself in the case of either.

My object in relating this case is to call out the views of members of the Society upon a point which I consider one of the most important and momentous in the whole field of obstetric practice; and before closing I desire to give, as concisely as I may, the reasons which actuated me in interfering in the case which I have brought to your notice.

First, let me state that I am fully aware of the fact, so patent to all, that a large proportion of women affected by puerperal albuminuria pass through labor without untoward symptoms, and rapidly recover after its accomplishment, and that I feel that I would be recording a mischievous and dangerous precedent did I by this history lead any one to interfere in the progress of utero-gestation merely on account of the development of this condition.

Albuminuria in the pregnant woman, then, let me premise, is insufficient as a ground on which to base premature delivery. In the case under consideration I was influenced and decided in my course by the following considerations:

1st. The patient's previous delivery and narrow escape from death from this very cause.

2d. The fact that even in the first month of her existing pregnancy she had given evidences of Bright's Disease.

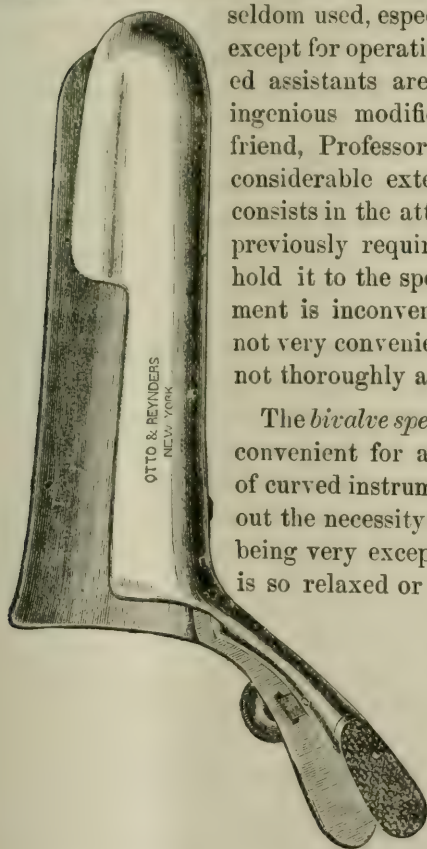
3d. The sudden appearance of grave cerebral symptoms coincidently with marked, even excessive amount of albumen in the urine, and, what I omitted to mention, tube casts filled with granular epithelium.

Believing now, (after having carefully reflected upon the case,) as I then did, that I was saving my two patients from a great danger by incurring a comparatively slight one, I should again, if similarly circumstanced, act as I did.

Modification of the Bivalve Speculum Vaginæ. By FREDERIC D. LENTE, M.D., of Cold Spring.

Among the various modifications of the *speculum vaginæ* which have been from time to time proposed, there is none which enables us to introduce into the uterus with facility the sound, when required to be much curved, or other curved instruments by means of which applications are made to the lining membrane, except the modification of Sims. This instrument necessitates the employment of an assistant accustomed to holding it in its place when applied, in order that one hand of the operator may be left at liberty. On this account it is now

seldom used, especially in private practice, except for operative procedures when skilled assistants are always at hand. The ingenious modification, however, of my friend, Professor Thomas, obviates to a considerable extent, this objection. This consists in the attachment of the retractor, previously requiring a separate hand to hold it to the speculum. Still the instrument is inconvenient to carry about, and not very convenient of application to those not thoroughly accustomed to its use.



The *bivalve speculum* is, perhaps, the most convenient for allowing the introduction of curved instruments into the uterus without the necessity of an assistant—the cases being very exceptional where the vagina is so relaxed or capacious as to cause its walls to fall between the blades and obscure the view, in which cases the trivalve is our next best resource. But when there is considerable flexion of the cervix uteri, as is frequently

the case, when we have disease requiring the application of

remedies through the whole extent of the cervical canal, or to the cavity of the uterus, it is necessary to curve the sound or other instrument very much, sometimes through the quarter of a circle or more; in this case it is very difficult of introduction on account of the small amount of room at the outer portion of the speculum, but little more in fact than we have in the tubular form. To obviate this difficulty, I propose the modification indicated in the accompanying engraving. That is, to cut away half of the width of the right hand or lower blade of the instrument, commencing at its proximal end, and extending a little more than half the length of the blade. This blade will still act more effectively in keeping the walls of the vagina apart than the retractor of Sims and Thomas, and gives abundant room for manipulation with curved instruments.

A further equally important modification proposed is to reduce the weight of the instrument nearly fifty per cent., and to make the handle much shorter, (but very slight force being required to dilate the vagina;) also to reduce somewhat the length of the blades, so that as little as possible shall extend beyond the vulva. These alterations allow the instrument to be held *in situ by the vagina itself*, and thus liberate *both* hands of the operator. To meet the case of relaxed and capacious vagina, and to obviate the necessity for resorting to the trivalve, it is proposed, also, as the engraving shows, to make the blades wider at their distal extremity than is usually the case. This can be done without materially increasing the difficulty of introduction through the ostium vaginæ, since they form, when closed, a tolerably thin wedge.

Medicinal Uses of Ptelea Trifoliata. BY O. F. POTTER, M.D.,
St. Louis, Mo.

I wish to call the attention of physicians to a plant, the medical virtues of which I have been familiar with for some years, and from personal experience would recommend it to the favorable notice of the profession.

The plant is known as the ptelea trifoliata, or, commonly, as the wafer-ash or wingseed, a species of so-called swamp dogwood, and is of the natural order Xanthoxylacea.

It is a shrub of from six to eight feet in height. The leaves are trifoliate and marked with pellucid dots. The leaflets are sessile, ovate, short, acuminate, downy beneath, lateral ones inequilateral; terminal ones cunate at base, from three to four inches long by one to one and three-fourths inches wide. The flowers are polygamous, nearly one-half inch in diameter, of a greenish-white color, and of a disagreeable odor. Stamens mostly four with style short; fruit, a two-celled samara, nearly an inch in diameter, winged all round, nearly orbicular. It flowers in June. It is common to this country, growing more abundantly west of the Alleghanies in shady, moist, and rocky places, generally at the edge of woods. The bark of the root possesses its peculiar medicinal properties, which it yields to boiling water, but alcohol is its best solvent. The bark, when dried, is of a light brownish-yellow color, comes in cylindrical rolls or quills a line or two in thickness, and from one to several inches in length; is irregularly wrinkled externally, and is covered with a thin epidermis; internally it is of a yellowish-white, but becomes darker on exposure. It has a peculiar, rather aromatic smell, and a bitter, pungent and rather acrid taste, yet nothing disagreeable; the pungency is persistent, which is owing to the oil which it contains.

I have been using it as a tonic to follow the use of quinine in all grades of fevers, also in cases of general debility connected with gastro-enteric irritation. It is mild, unirritating, having a soothing influence on the stomach, promoting digestion. It promotes the appetite, enabling the stomach to endure suitable nourishment, and favors the early re-establishment of the digestive functions, and will be tolerated by the stomach when almost all other tonic or stimulant remedies are rejected. I have found it especially useful in cases of debility following a low grade of fevers, also with females after confinement, or where the menstrual functions are deranged, frequently by sustaining the digestive and secretive functions, regulating the menstrual flow; also as a sustaining and strengthening stimu-

lant in debility connected with or following wasting ulcers or scrofulous sores.

I have been in the habit of using it in the form of a tincture, made by putting six ounces of the bark and one-half ounce of ginger to two quarts whisky; the dose from one to two table-spoonfuls three times a day for an adult.

I feel assured, from over ten years' experience in using it, that it will be found a most valuable and reliable remedy. It has been used occasionally by the so-called eclectic physicians, and also by the negroes of the South, who call it the scrofula root, from its usefulness in sustaining the system when debilitated by that so common disease amongst them. The old French inhabitants near St. Louis also used it many years ago as a cure for the intermittent fevers of the country, long before quinine was known. When used for a great length of time, or in very large doses, it occasionally, in some persons, occasions an erysipelatous inflammation in the surface, which, however, only lasts for a short time if its use is persevered in, and no ill effects follow it.

PROCEEDINGS OF SOCIETIES.

NEW YORK PATHOLOGICAL SOCIETY.

Stated Meeting, June 14, 1865.

DR. GURDON BUCK, President, in the Chair.

BULLET REMOVED FROM RAMUS OF JAW—DR. A. C. POST.

Dr. Post presented a flattened ball removed from the left ramus of the jaw of a soldier who had been shot at Coal Harbor. The ball entered at the junction of the right ala of the nose with the upper lip, passed across the mouth, and was lost. There was a large swelling in the parotid region, and also a sinus which communicated with dead bone. In the course of the operation the bullet was felt and removed, along with a molar tooth of that side, to which roughened and dead bone was attached. The movements of the jaw, which were very much interfered with before the operation, were afterwards much improved.

WOUND OF BOTH CAROTIDS—DR. POST.

Dr. Post also presented the atlas and axis removed from a soldier who died from hemorrhage seven days after a gun-shot wound which he had received during the month of February last. The ball, which was a Minie, entered the mouth, passed through the tongue, and was lost. The patient did very well for six days, but on the seventh the fatal hemorrhage referred to occurred. At the autopsy the bullet was found lodged upon the upper surface of the transverse process of the atlas, having perforated in its course the external and internal carotid arteries. Dr. Post stated that it was certainly remarkable that two such large vessels should be thus wounded, and yet not bleed for so long a time as six days.

Dr. KRACKOWIZER thought that the arteries were not perforated at first, but only became so as the result of the slough which followed the original injury to their coats.

Dr. CONANT remarked that he had heard, from a general, that the rapid passage of a bullet through the air caused the material of which it was composed to become very soft, and that this fact offered a very reasonable explanation for the frequent flattening of these missiles in parts of the body remote from bone or other hard substance.

Dr. Post stated that the flattening, under such circumstances, could generally be explained by the missile striking a rock or tree before entering the body. He had removed a bullet very much flattened from the walls of the abdomen which had entered in the neighborhood of the nates, and traversed a distance of nine inches under the skin, and had not in its course infringed upon any hard substance. The wound was received during the battles of the Wilderness, and it was fair to presume that the ball had struck a tree first.

Dr. CONANT remarked that a few days before he had occasion to remove a Minie ball which was situated immediately over the femoral artery, and which was flattened in such a curious manner that he could not explain its condition, except on the supposition that the theory of the general was correct. It seemed that the ball had been flattened simply by coming in contact with the tendon of the abductor muscle, and he supposed that it was not flattened before it entered the body, because the wound of entrance was disproportionately small.

Dr. Post remarked that wounds of entrance generally showed a strong disposition to retract, under any circumstance.

Dr. Geo. T. ELLIOT stated that the question concerning the theory of the flattening of bullets could very easily be settled by an observation of the state in which spent balls are found.

UTERINE TUMORS—DR. H. B. SANDS.

Dr. SANDS exhibited a mass of uterine tumors, and gave the history of the case, as follows:

It is my painful duty to exhibit to the society a specimen obtained from a woman, whose death was caused by a surgical operation to which she submitted by my advice. She first came under my notice about three months ago, having been sent to me as the supposed subject of ovarian disease, by a medical gentleman in this city, with a view of having an operation performed. The patient was an unmarried woman, 45 years of age, and of healthy parentage. She belonged to a long-lived family, and having a disease which she supposed would terminate her life in a short time, felt a natural anxiety to be cured, if possible, and to live as long as her ancestors had. I examined this woman with regard to her history, and I found that an abdominal tumor made its first appearance seven years before; the patient was very positive in saying that it first showed itself in the left iliac fossa; that the growth was at first gradual, but that in a year's time it had attained a very considerable size. Just at the time of consulting me her particular grievance was the weight of an umbilical hernia, in the sac of which was accumulated a considerable amount of peritoneal fluid. She represented to me that her health was suffering severely; that life, under the circumstances, had no attractions for her, and that she desired to have an operation performed. An examination of the tumor led me to suspect that it was not of ovarian but of uterine origin, although certain facts in her history and in her physical examination pointed strongly to the existence of ovarian disease. On inquiry I found that menstruation had never been excessive, but that occasionally she had suffered from a suspension of the flow; at no time had there been any thing like uterine hemorrhage.

The enlargement of the abdomen was very great. I did not measure it myself, but the measurement was made by my friend, Dr. Sabine, at the N. Y. Hospital. He found the greatest measurement around the umbilicus to be forty inches. The umbilical opening was circular in shape, two inches in diameter, and there was considerable distension of the sac. In order to prevent the protrusion of this hernia the patient had been obliged to wear a truss, and it was this inconvenience, added to that caused by the tumor, which led her to seek relief from an operation.

This set of tumors (referring to the specimen) could be partly made out through the abdominal walls, which, by the way, had no very considerable thickness. I could distinguish this swelling, which is

a fibrous tumor springing from the fundus of the uterus, immediately below the umbilicus. The uterus itself, which lies beneath, I could not distinguish through the parietes of the abdomen. I was able to feel a swelling of considerable firmness in the left iliac fossa, and a much larger one, in fact, the largest of all, existed above and to the right. The tumor allowed an examination through the open umbilicus, and the larger and smaller swellings could be separately distinguished. In regard to fluctuation, I was not certain. It is true that fluctuation existed, but the presence of some dropsical fluid in the abdominal cavity so masked the feeling of deeper fluctuation that, as I said before, I could not be positive about it. I made a vaginal examination, and with great difficulty was able to reach the os; there was no vaginal cervix, for the reason that the uterus had ascended so far from the outlet of the pelvis. The uterus was very decidedly to the left of the median line; I was confirmed in this opinion by the uterine sound, which took a direction upward and to the left. The instrument penetrated to the depth of $3\frac{1}{2}$ inches, and, during an examination which Dr. Krackowizer had the kindness to make afterward, it penetrated about 4 inches. The instrument went up and to the left, and there did not seem to be any considerable thickness between the finger outside and the point of the instrument within the uterus. On endeavoring to move the uterus by rotation of the sound it was found impossible; but on taking the handle of the sound in the left hand, and moving the mass upon the left side with the right hand, a distinct movement was communicated to the handle of the instrument. This made me believe in the existence of a uterine rather than an ovarian tumor. I subjected the woman to several examinations, but was not able to arrive at any more satisfactory conclusion. I was induced to operate partly on account of the woman's urgent request, and partly because I thought it very manifest that her health was suffering from the burden of this swelling and the additional weight of the hernial protrusion, and also because I had made up my mind that uterine tumors could be successfully extirpated. I was led to this conclusion by reading the two cases given by Mr. Clay, of Manchester, and the one by Koiberly, of Strasburg. I omitted to state that there was very decided mobility of the abdominal swelling. The woman went to the hospital and was submitted to a formal consultation; an operation was decided upon, and this I performed on the 12th inst.

The patient being placed under ether, the operation was commenced by an incision in the median line, about $3\frac{1}{2}$ to 4 inches in extent, below the umbilicus. The abdominal cavity was reached with great facility.

I introduced my right hand, and swept it over the left surface of the tumor, and with my left hand felt the right surface of the tumor, and could detect but a single adhesion of about three-quarters of an inch in extent to the greater omentum. I should state that at this time the diagnosis was still uncertain; it was not evident to those who saw the swelling through the incision what was the nature of the tumor. It was considered right to prolong the incision upward. I accordingly enlarged the incision, and thought it necessary to make a very long one, nearly up to the summit of the tumor. That being done, I was able, by placing my hand behind it, to dislodge it from the abdominal cavity, and bring it into view. It then did not appear to be a fibrous tumor, and I was so much in doubt of its character that I tapped it; but no fluid was found. To my great disappointment, on passing down into the pelvic cavity I found that my hand was arrested by large and firm adhesions. My impression was, at this time, that the further prosecution of the operation would be fatal to the patient; yet I readily yielded to the advice of my colleagues to proceed with the extirpation of the tumor, as I had reason to believe, with the other gentlemen present, that if the mass were returned the woman would certainly die, while by the removal of the mass she might have a chance of life. We accordingly commenced the dissection, which was a very difficult operation, as step by step we were met by dangers in the shape of blood-vessels of various sizes, large and small. Towards the last the operation had to be hurried, as the woman showed signs of fainting; and after much trouble, and a great deal of embarrassment, this tumor was finally removed. The mass was removed above the line of implantation of the vagina into the cervix uteri. As soon as this was completed, it became evident that there was very alarming exhausting hemorrhage. The sponges were removed, and pressure made upon the bleeding points, and these points were secured as fast as possible. The main hemorrhage was found to come from a rent in the common iliac vein. The summit of the bladder was unavoidably ruptured; it was, however, closed again by the application of a ligature. The operation consumed about one hour and a half, and, notwithstanding the free use of stimulants, the patient sank and died a few minutes after its completion. Death, in my opinion, was mainly due to hemorrhage, taking place from large blood-vessels in the pelvic cavity, which were unavoidably lacerated during the operation. Regretting, as I do, the unfortunate termination of the case, I am still inclined to regard the disaster as one which might happen in the extirpation of an ovarian as well as a uterine tumor, and can see no reason why, in the

absence of deep-seated, extensive adhesions, the removal of the uterus might not be accomplished with a favorable result.

An examination of the mass removed shows it to consist of the uterus and appendages, the former being the seat of a large number of fibrous tumors, some of which are developed in its walls, the largest, however, forming nearly independent growths, connected with the uterus only by long narrow pedicles. The uterus itself is nodulated externally, but preserves nearly its regular figure; it is greatly hypertrophied, however, measuring nine inches in its longest diameter. The uterine sound can be made to pass over seven inches through a somewhat tortuous canal. The mucous membrane lining the interior is slightly thickened and vascular, but otherwise healthy. The principal outgrowths from the uterus are three in number, one attached to the summit and one to either side of the body of the organ—that upon the right side being the largest. On section these tumors exhibit the usual appearances of the softer variety of fibrous growth, resembling in many places œdematous areolar tissue. The largest tumor—that in the right side—is the seat of several cysts containing serum. The mass, three days after removal, weighed sixteen pounds, but as it diminished considerably in bulk from the loss of fluid during the interval, it is estimated to have exceeded twenty pounds in weight at the time of the operation.

OVARIAN TUMOR—DR. GEO. T. ELLIOT.

Dr. GEO. T. ELLIOT presented a specimen of ovarian tumor, with a written history.

(For a full report of this case see the September number, p. 409.)

Dr. CLARK stated that he had seen the patient on several occasions, and the last time he had come to the conclusion that there was nothing to hinder her recovery. She had no well marked signs of peritonitis at any time, and during all the period which she was visited by him there were no symptoms that excited any special anxiety. In conclusion he stated that he would like to add one fact in regard to the statements of Dr. Sands in regard to the removal of the uterus. About fifteen years ago Dr. Kimball, of Lowell, operated upon a female in some uncertainty whether he had a uterine or ovarian tumor. It proved to be a fibrous tumor of the uterus; he removed the organ and its appendages, and the patient made a good recovery.

Dr. CONANT referred to a uterine tumor that had lately been removed in Boston, but the patient survived the operation only twelve hours.

Dr. KRACKOWIZER was inclined to think that the diarrhœa which terminated the life of Dr. Elliot's patient was purely of accidental character, and unconnected with septicæmia. In regard to Dr. Sands' case, he believed that uterine tumors could be removed with safety, the only difficulty being to decide whether or not there were any extensive adhesions to the pelvis.

Stated Meeting, June 28.

Dr. GURDON BUCK, President, in the Chair.

CHRONIC INTESTINAL OBSTRUCTION—DR. H. B. SANDS.

Dr. SANDS exhibited several specimens taken from the body of a young man who had suffered from the effects of chronic intestinal obstruction during life, and in whose case he had performed Amussat's operation for the formation of an artificial anus.* The operation was done on the 9th of January, and afforded at the time complete relief from the distressing symptoms which preceded it. The swelling of the abdomen subsided, the pain disappeared, and the feces escaped readily through the artificial opening. Meanwhile the patient gained strength, his appetite improved, and, towards the latter part of January, he was able to get up and walk about the hospital wards.

For about ten days after the operation, the discharge of feces through the wound was nearly constant, but subsequent to this it was intermittent and irregular, sometimes taking place spontaneously, at other times by the aid of an enema of warm water, or after the administration of a laxative. The artificial anus showed the usual tendency to contraction, to obviate which, as well as to prevent the involuntary escape of fecal matter, the patient wore an instrument, constructed at my request by Messrs. Tiemann & Co., consisting of a cylindrical plug of hard rubber attached to a plate of the same substance, which covered the neighboring skin, the whole being retained in position by tapes passing round the waist. This apparatus answered its purpose admirably, and was worn for some time with great comfort; its use was finally abandoned, however, in consequence of the orifice of the intestine becoming inflamed by the repeated introduction of the syringe.

On the 27th of February the patient had so far improved that he left the hospital and paid a visit to his friends in Rhode Island. On

* Case reported in the April number of NEW YORK MEDICAL JOURNAL, 1865.

the third of March following, however, he returned to the hospital, complaining again of pain, and a feeling of abdominal distension. Feces were discharged in small quantity, and at irregular intervals, from both the natural and artificial outlets, yet the relief thus afforded was incomplete. A careful exploration was again undertaken, and an attempt made to dilate the strictured intestine by an elastic bougie introduced through the artificial anus. The effort was unsuccessful, the instrument being suddenly and completely arrested at a point opposite to the situation of the internal abdominal ring, and about eight inches from the lumbar opening.

The subsequent progress of the case may be briefly told. The symptoms were mainly those which had existed previous to the performance of Amussat's operation, and pointed to the presence of mechanical obstruction at some *other* point than that for which the operation had been undertaken. Gradually increasing abdominal distension, obstinate constipation, loss of appetite, emaciation, and paroxysms of intense pain finally exhausted the patient, who died rather suddenly in the night of May 1st, less than four months after the operation.

Autopsy, thirty hours after death. Body extremely emaciated; abdomen somewhat distended, and tympanitic. Head not examined. Thoracic organs healthy, except the right lung, which presented, near its apex, two masses of tubercular deposit, each one being a little larger than a hazel-nut. In one of these masses the morbid product was in process of softening; in the other, it had undergone calcification.

On opening the cavity of the abdomen, small yellowish-white nodules and patches were found scattered abundantly beneath the peritoneum lining the abdominal walls, and that covering the large and small intestine, and also in the substance of the great omentum. These, on subsequent examination, proved to be tuberculous. The omentum was misshapen, folded upon itself, and adherent to the small intestine. The coils of the latter were universally adherent to one another, the union being in some places so firm as to resist all attempts at separation. Immediately above, and in the upper third of the pelvic cavity, the coats of the small intestine were greatly thickened and indurated, from chronic inflammatory deposit, so that its convolutions formed a nearly solid mass, which was firmly adherent to the posterior abdominal wall, and to the anterior surface of the rectum. The coats of the latter exhibited the same kind of thickening and induration, especially just above the promontory of the sacrum, where its calibre was

so much diminished as hardly to admit the tip of the little finger. In all that portion of the small intestine included in the mass above described, a portion embracing several feet of its length, the canal was narrowed in its diameter, and in three or four places was only large enough to give passage to a full sized steel bougie. Absolute obliteration of the intestinal canal was, however, nowhere to be seen. The large intestine, with the exception of the lower part of the sigmoid flexure and the rectum, was but slightly thickened, and its calibre was normal. A careful examination failed to detect any evidences of ulceration of the intestinal mucous membrane, either past or present. The artificial opening in the colon was found to have been made quite behind the peritoneum, and no traces of inflammation were observed that could be attributed to the operation. The results of the post-mortem examination render it evident that the symptoms presented by the patient during life were due to chronic intestinal obstruction, caused by tubercular peritonitis. It is also evident that the obstruction made its appearance in the lower part of the sigmoid flexure of the colon at a time when the passage of fecal matters through the small intestine was tolerably free, so that the formation of an artificial anus afforded complete relief immediately, and for some time after the operation. Gradually, however, the disease extended to the small intestine, and gave rise to mechanical obstruction which, not admitting of further surgical relief, necessarily hastened to a fatal termination.

VESICAL CALCULUS—DR. D. S. CONANT.

Dr. CONANT presented a vesical calculus weighing 645 grains, which he removed from a gentleman in Brunswick, Maine. A year ago last spring a gentleman presented himself at the college with severe trouble in the bladder, which had been designated as chronic inflammation, he having been under treatment for that affection for six months previously. On careful examination with the sound, a stone was found in the bladder, but the patient not being ready for its removal, the operation was deferred until the 26th of April last. The operation was performed by a single lateral incision on the left side, and lasted but little over four minutes. In two weeks after the operation the patient was out riding, and in three weeks he was able to be about the village. The patient did not wear the catheter for a single day at a time after the operation, although the instrument was daily introduced for two weeks in order to see that the healing was going on rightly.

OSTEO-MYELITIS—DR. WEIR.

Dr. WEIR presented a femur which he had removed from a soldier, a patient of St. Luke's Hospital, on the 7th of June, by amputation at the hip-joint. The patient was wounded at the first battle of Frederick, December 18, 1862, by two balls which entered the right thigh; one of these, a minie, fractured the upper and middle third, and a number of large fragments of bone were removed. He was transferred from one hospital to another, until he finally entered the St. Luke's, on the 9th of June, 1864. From the date of his entrance into the latter hospital, several abscesses had formed which communicated with the seat of fracture, though no dead bone at that point was felt. He kept about on crutches until October, 1864, when he was compelled, by a repetition of abscesses, to go to bed. These abscesses were discharging, more or less, all the time well formed pus. In May, when Dr. Weir went on duty, a new abscess formed, running up towards the trochanter, and the patient then commenced rapidly to run down. A consultation was called, and it was decided to remove the thigh by disarticulation, which was accordingly done by the method known as Van Buren's. Patient, at last report, was doing well. The specimen was a very interesting and beautiful one of osteo-mylitis. In the substances of the head of the bone there were two small abscesses which had nearly made their way through the thin crusted surface. There was never any complaint of inflammation in the knee-joint. About six ounces of blood only were lost during the operation in consequence of the application of the clamp tourniquet to the aorta.

Dr. BUCK thought that the case was a very favorable one for operation, and, in fact, afforded a good illustration of the good effect of an operation which was performed for chronic disease of a limb.

SALIVARY CALCULUS—DR. L. A. VOSS.

Dr. Voss presented a salivary calculus, removed from the left Wharton duct of a patient 27 years of age, who came to him complaining of pain in the submaxillary region. On examining the mouth, Wharton's duct was seen very much distended, and its orifice was very prominent. On introducing a fine probe a slimy fluid mixed with pus escaped, and the instrument soon struck upon a hard substance which proved to be the calculus, and which was removed by a simple incision. This calculus was confined altogether to the duct, and was of course more easily removed than if it had been in the substance of the

submaxillary gland. These concretions were mostly made up of phosphate of lime and chloride of soda.

Dr. Post remarked that concretions in all the mucous glands were for the most part made up of phosphate of lime.

In answer to a question from Dr. Buck, as to the size of these calculi, Dr. Voss stated that he had seen six cases of the sort, and in all the concretions were of large size, except in one instance. Dr. Detmold had one case where the calculus was the size of an almond; another one which Dr. Voss had seen, and which was removed by piecemeal through a fistulous opening was as large as a cherry. It was a curious fact for him to notice that all the salivary concretions which he had seen were connected with the submaxillary gland. He further remarked that these concretions were not unusual in horses, the nuclei of which were formed from some portions of the fodder. In conclusion he referred to a case of concretion in the submaxillary gland which occurred in the practice of Dr. Krackowizer. A probe passed into the duct could not detect the calculus, but a needle passed from the inside into the tumor, struck the hard substance. An attempt was made to remove the stone by an incision on the inside of the mouth, but failed in effecting the object; but finally suppuration and ulceration in the parts took place, and the stone was discharged in fragments.

Dr. CONANT stated that he presented, two years since, a calculus from Wharton's duct, measuring an inch in length, $\frac{3}{4}$ inch in width, and $\frac{3}{8}$ inch thick.

ANEURISM OF THE AORTA—DR. A. L. LOOMIS.

Dr. LOOMIS presented a specimen taken from a German 29 years of age, who was admitted into Bellevue Hospital on the 3d of June. He stated, on his admission, that he had been well up to five years ago, when he had an attack of inflammatory rheumatism, which confined him for about six weeks, after which time he was well up to six months ago, when, after making an exertion in going up stairs, he noticed that he had shortness of breath, with palpitation of the heart. Three months ago he noticed that his feet began to swell, and at this time his attacks of dyspnoea became more frequent and severe. On admission, his general appearance indicated that he was suffering from organic disease; his countenance was anxious, his pulse 80 and jerking, but his mind was clear. On physical examination, bronchial rales were heard on both sides; there was no dullness on percussion. The heart was found hypertrophied, the apex beating about an inch and

a half to the left of the left nipple. On auscultation, three distinct murmurs were heard, one at the apex of a blowing character, and one to the left of the apex and behind at the angle of the scapula. Another following the first sound, of a different character, heard at the base and conveyed along the artery; and a third at the base, and conveyed downwards and to the right, following the second sound, and heard at the junction of the third rib with the sternum on the right side, being harsh in character. From this time his dropsical effusion went on rapidly, his feet and lower extremities became more œdematous, extending up so as to involve the scrotum, yet the rhythm of the heart was perfect up to the time of his death. The murmurs were distinctly recognized, and they were so different in character, the two murmurs following the first sound, that any one could appreciate it. He died from sloughing of the integument of the lower extremities. At the autopsy, all the organs were found healthy, except the heart and kidneys. It is proper to state that granular casts were found in his urine, which explained the advanced stage of disease with which the kidneys were affected. They weighed together 18 oz. The left side of the heart was markedly hypertrophied, the walls of the ventricle being $\frac{3}{4}$ of an inch in thickness. The left cavity was dilated, but not very extensively so. There was found a fibrinous deposit, which was pretty well organized, upon the endocardium of the left ventricle, and also upon the commencement of the artery. The aortic valves were thickened, and were slightly insufficient, the mitral valves were also thickened but were not insufficient, and it seemed to Dr. Loomis that they were not sufficiently diseased to give rise to the murmur which was heard at the apex. At the commencement of the aorta was one of those unrecognized and unrecognizable aneurismal sacs, with an opening large enough to admit the end of the little finger, and capable of containing about two ounces of fluid. This sac had not been ruptured. The point of interest with Dr. Loomis was, how much the sound, which was heard at the apex, was due to disease at the base?

Dr FLINT thought, that inasmuch as the murmur with the second sound was heard to the right of the sternum, which is not true as a sign of regurgitation, it might be suggestive of the existence of an aneurism.

Dr. KRACKOWIZER, in this connection, thought that the aneurism pointed towards the right side.

Dr. LOOMIS presented a second specimen taken from a young German girl, who was received into the Hospital on the 12th of June.

When she was admitted she was, to all appearances, in perfect health. She stated that she had been well up to a month before, when she became disabled on account of a pain in her chest and limbs, and that this pain had compelled her to remain in bed. On examination, her pulse was found to be 100, and very nearly natural in character; her respiration was slightly hurried, and she complained constantly of a sense of constriction and pain across her chest. The physical examination revealed a murmur, and this murmur was the interesting point in her case. It was heard at the apex, following the first sound, and heard with equal intensity at the angle of the scapula behind. There was also a different sound at the junction of the fourth rib with the sternum, preceding and following the first sound; this was of harsh character, and it resembled so closely the friction sound that it was mistaken for it, and a diagnosis was accordingly made of mitral regurgitation with pericarditis. This case was also examined by Dr. Flint, who recognized a mitral obstruction, and a mitral regurgitation murmur.

Dr. FLINT stated that he had a recollection of examining the case and recognizing a mitral obstructive murmur, but remarked, in that connection, that such a murmur always ceased with the first sound of the heart.

Dr. CLARK called to the mind of the Society that he was placed upon a committee to report upon the question as to whether, in diseases of the heart, the murmurs ceased to be heard when the patient was suffering from pneumonia; and as that committee had been instructed to report within four months, and as that time would expire before the next meeting, he ventured to make an informal report, in the absence of the other gentlemen composing that committee.

He stated that he had seen but three cases of pneumonia with heart disease. In one of them the heart sounds continued from beginning to end, and in two the heart sounds continued all the time that the pulse did not exceed 120 per minute. In three or four instances of heart disease where the patients were suffering from other troubles than pneumonia, and where the pulse was accelerated, the murmurs ceased to be heard, and this afforded the true explanation for the phenomenon, viz.: that any disease which sufficiently accelerates the pulse will, in patients already suffering from heart disease, render the murmurs inaudible.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Practice of Medicine and Surgery applied to the Diseases and Accidents incident to Women. By WM. H. BYFORD, A. M., M. D., author of "A Treatise on the Chronic Inflammation and Displacements of the Unimpregnated Uterus," and Professor of Obstetrics and Diseases of Women and Children in the Chicago Medical College. Philadelphia: Lindsay & Blakiston.

Prof. Byford has reason to congratulate himself on the favorable and very well deserved reception that has been accorded to his "Treatise on Inflammation and Displacements of the Uterus," and the present volume from his pen is worthy of attentive critical examination. The publishers have done their part well. The paper, type and binding are in every way acceptable. In the preface we are informed that the work is written "for the student and junior practitioner;" but we are not informed of the fact that the present volume contains the whole of the "Treatise" referred to, *verbatim et literatim*. Of the 555 pages in the present volume, 193 are reprinted from the author's previous work, leaving us but 362 to consider in the present review. They treat of the following topics: Chap. I. Diseases and Accidents of the Labia and Perineum. Chap. II. Diseases of the Vulva. Chap. III. Vaginitis. Chap. IV. Menstruation, and its Disorders. Chap. V. Menorrhagia. Chap. VI. Dysmenorrhœa. Chap. VII. Metatithmenia, (*Μετατιθήμι μην*), or Misplaced Menstruation. Chap. VIII. Acute Inflammation of the Unimpregnated Uterus. We now pass over to the middle of Chap. XXII., where, in the consideration of Displacements, their Philosophy and Treatment, the latter half of the chapter is devoted to Inversion of the Uterus, a topic not studied in our author's previous work. Chap. XXIII. Diseased Deviations, or Involutions of the Uterus. Chap. XXIV. Cancer of the Uterus. Chap. XXV. Tumors of the Uterus. Chap. XXVI. Ovarian Tumors. Chap. XXVII. Diseases of the Mammæ. Chap. XXVIII. Phlegmasia Alba Dolens, or Crural Phlebitis. Chap. XXIX. Puerperal Fever. Chap. XXX. Stomatitis Materna, or Nursing Sore Mouth.

These important topics are all handled vigorously and practically, and the earnest, energetic desire of the author to advance the best interests of his patients and the profession is seen in every page. The work can not fail to be in great demand, and to exert a material influence on clinical practice throughout the country. While we cordially recommend the book to the "student and junior practitioner," as well as to

specialists, we are convinced that such a general and hearty recommendation needs certain qualifications; and when we consider the deservedly high position which the author holds as a practitioner, a teacher, and an author, we feel warranted in offering a running commentary on such portions of the book as seem to us deficient in the best clinical recommendations, without incurring thereby the risk of being misunderstood. Such comments are not intended to detract from the sincere and hearty favor with which we welcome and recommend the book, or to be accepted as any evidence or indication that the points to which we except in this brief article are other than exceptional in the book itself. The author and the profession will decide whether, after all, the views of Dr. Byford may not be better than those of the reviewer.

In Chap. I. the subject of Rupture of the Perineum is considered, from an operative point of view, in strict accordance with the well known principles inculcated by Mr. Baker Brown. For this gentleman, indeed, our author entertains a sincere deference, as may be seen in several allusions scattered through the work itself; and he states that, "for the most destructive form of rupture, the operation taught by Mr. Baker Brown is so perfect, and has been so entirely successful in my own hands, that I will not apologize for recommending and describing it without variation" Dr. Byford has also reproduced the well known plates of Mr. Brown illustrative of the operation.

Now it is the conviction of the reviewer that the operation which has been performed for the last ten years in this city by Dr. Sims and Dr. Emmet is a much better operation than that of Mr. Brown's, and moreover, in an American book, it would certainly have been well to have recognized an American operation. The objection to Mr. Brown's operation is this, viz., that division of the sphincter is not at all necessary, much less the double division inculcated by Mr. Brown and Dr. Byford. Division of the sphincter ~~is~~ is a very simple matter; the safety as regards blood-vessels, and the secure pads of fat in the ischio-rectal fossæ encourage the inexperienced operator; but we are convinced that in many cases the subsequent contraction of the cicatrices interfere with due retention of gases and fluid feces, and we appeal to those surgeons who have followed up their cases, and to those who have been the subject of the operation, whether these considerations are not of weight. Now the operation of Dr. Sims and Dr. Emmet discards division of the sphincter; and, at the request of the reviewer, Dr. Emmet has kindly furnished the following brief abstract of their operation and its results.

"In the operation for closing a lacerated perineum, either partially

or entirely through the sphincter ani, it is unnecessary to divide the muscle or to make incisions into the soft parts for the purpose of relieving tension.

"As early as 1855 Dr. Sims, in the Woman's Hospital, simplified this operation by bringing the scarified edges of the laceration together by means of deep interrupted silver sutures, and from this time the use of the quill suture, or a division of the sphincter ani, has been abandoned. Further experience demonstrated a necessity for the use of a short rectal tube for some ten or twelve days after the operation, that a free escape of flatus might be unobstructed. Where the laceration of the perineum has extended only to the sphincter, the rectal tube is not needed, and three interrupted sutures are generally sufficient; if more extensive, so as to involve the muscle, two in addition are required. The first suture passed should be the one nearest to the rectal mucous membrane, and should be made to follow the laceration entirely around, so as to bring together the sphincter. The second should also include the sphincter, and be passed in the recto-vaginal septum, just beyond the first one. The remaining sutures are introduced (as in the operation for a partial laceration of the perineum) through one labia about half an inch from the edge on one side, introduced from within outward into the other, and withdrawn at a point equally distant, so as to approximate perfectly apposite surfaces. If the laceration has extended up the recto-vaginal septum for some distance beyond the sphincter ani, the edges should be brought together down to the sphincter by interrupted silver sutures, at a distance of about five sutures to the inch. On introducing the first suture to clear the perineum, care must be taken that it is passed between the first and second sutures uniting the septum, and the next one in turn between the second and third. Without this precaution an opening into the vagina will be produced just behind the sphincter, from the fact that, as one set of sutures is passed at a right angle to the other, on twisting those of the perineum, tension would be exerted. This is a weak point, for if the tube is allowed to become obstructed, a small recto-vaginal opening will always result from the escape of flatus in this direction. I always scarify by means of scissors; it can be done rapidly, and with less hemorrhage. The knees should be kept tied together ten days after the operation, and the urine drawn with care, so that none is allowed to escape over the surfaces brought in apposition.

"The sutures of the perineum are usually removed about the sixth day; those within the vagina must remain for two weeks or longer, until the parts are strong enough to admit of the introduction of a

speculum. The bowels are to be constipated for two weeks at least in all cases where the sphincter has been lacerated. When the bowels are acted on by either a purgative or a warm mucilaginous injection, the success of the operation will greatly depend on the dexterity of the nurse in properly supporting the parts.

"During the past ten years, in the hospital practice of Dr. Sims and my own, this operation has been uniformly successful. There has not been a single case of failure in uniting the sphincter and perineum by the first operation. I have, however, occasionally partially failed in private practice, where the laceration has been extensive, from a want of care in keeping the rectal tube unobstructed, or in not properly supporting the parts during the evacuation of the bowels, and necessitating a subsequent operation to close the rectal opening."

We also dissent from the brief recommendations of the author regarding the operation proposed for "complete prolapse of the uterus." In the majority of these chronic cases, simple restoration of the perineum will merely palliate for a time. If the tone and calibre of the vagina be not restored, the uterus will sooner or later distend and relax the newly made perineum so as to reproduce the prolapse.

The chapter on vaginitis is especially good in its practical suggestions regarding the treatment of the puerperal form and the fistulæ associated therewith. In the treatment of the acute form, especially in that due to gonorrhœa, no allusion is made to the practice of leaving within the vagina lint or cotton saturated with medicated fluid in the intervals between the injections, a practice which we believe to be very beneficial. Without desiring to be hypercritical, we regret that in an American work the undoubted claims of Dr. Sims to the *originality* as well as the popularization of the operation and instruments employed have not been more clearly stated for the information of the student. Prof. Syme, in his address before the British Medical Association, has lately ranked the labors of Dr. Sims with those of the introducers of sulphuric ether.

While discussing amenorrhœa, Dr. B. sets forth very happily the action of the so-called emmenagogues, "as producing their effects in two different modes; one is to cause the growth and discharge of the ova, and the other the discharge of blood as a hemorrhage. It would be better, then, to say that they were oviferous in their nature in the first case, and hemorrhagic in the second."

In the chapters on Menorrhagia and Dysmenorrhœa the recommendations for full dilatation of the os and cervix so that the finger can be introduced to the fundus, and the subsequent thorough

cauterization of the mucous membrane of the cavity of the uterus with the acid nitrate of mercury, are adapted to do a great deal of harm in less experienced hands, if followed closely. Indeed, we venture the prediction that if Dr. B. will keep a record of his cases, he will not long continue the treatment without unfavorable complications. Metritis, perimetritis, and possibly metro-peritonitis will attack a certain proportion of cases similarly treated. We do not object to the treatment itself in experienced hands and well selected cases, but do not like to see the practice recommended with so little caution as to the risks involved.

Dr. Byford makes three divisions of Dysmenorrhœa, viz., inflammatory, neuralgic and membranous. He classes obstructive dysmenorrhœa under the first division, and makes a further division between painful menstruation and menstruation attended with pain; and states, regarding the latter class, that any pelvic organ "may become very painful by having the inflammation exaggerated as a consequence of the menstrual molimen, or any pelvic organ with strong neuralgic proclivities may become affected severely by the hyperæsthesia of that period." The views of our author regarding membranous dysmenorrhœa are decided as to the existence and recurrence of a membrane without conception. He is convinced that "it will not be long until (*sic*) the profession will review this matter and exclude obstructive dysmenorrhœa from the list of uterine ailments, or else find it to be exceeding rare in its occurrence." We believe, for our own part, that the tendency to divide the cervix has been carried further than future experience will justify, and that many who have been prominent in a resort to this operation will hereafter perform it less frequently; we believe that in the vast majority of cases, while the operator has assumed that the obstruction existed at the os internum, and that this stricture was divided, there has been no other effect than division of the vaginal portion of the cervix. We know that the healthy os internum is always very small, and that it can not ever be divided without very great risk of hemorrhage, and that the men who divide the cervix in their office, and let their patients walk home afterwards, either conceal their knowledge of alarming and fatal results, or else simply divide the vaginal portion of the cervix. Still we believe that the profession will never so far review their decision as to exclude obstructive dysmenorrhœa from their classification; we believe that division of the cervix, and even of the os internum in very rare contingencies, will always be a proper procedure in well selected cases, and that such division will increase the chances of pregnancy in many a

sterile woman. This advantage does not appear to have been alluded to by Dr. Byford. We venture to say, however, that very many an operation on the cervix has been performed on account of the conoid shape of the cervix, by men who did not appreciate that such was the normal shape of the nulliparous cervix, and that the only conditions warranting the incision are those in which the tissue is the site of chronic inflammation, or where obstructive dysmenorrhœa is *proven* to exist. We are glad, however, to recognize the influence of Dr. Byford in counteracting the tendencies towards an operation that is probably performed without any proper indication or satisfactory result as frequently as any other for which we have a name.

Alas! in Chapter VII. Dr. B. has exercised the right of every popular author in coining a word, and in resorting to the Greek dictionary for its derivation. Metatithmenia is the result of his labor, and implies misplaced menstruation. The unfortunate student has this one load more to carry; the practitioner who conceals with effort a tendency to stammer has one more trap set for him, and gynæcologists d'outre manche recognize in the Greek θ their old stumbling block in the pronunciation of English. Why would not some Indian dialect have sufficed? Metamora is more euphonious than Metatithmenia, or if the "men" be essential, why not keep some melodious name in view, like that of Mendelssohn? We are tempted further to say, in this connection, that before coining new Greek words, we would have preferred to see more attention to many of the Latin prescriptions in a work from an educated man prepared especially for the use of students.

We now pass to page 305, to the consideration of the subject of inversion of the uterus, which, like the other topics, is well and ably considered. A single cardinal omission will, we hope, be supplied in another edition, viz., the advantages to be derived from conjoined manipulation, with one hand or part of a hand in the vagina and one over the abdomen, in both diagnosis and treatment. In respect to the replacement of chronic inversion the Doctor very correctly states that "the great thing to be gained is the commencement." After or even before this is gained, we suggest that there is a great advantage in having the operator relieved rapidly by two or three experts, as the hand of one man in this reposition soon becomes too tired to be of much service. We give the preference to the hand over any instrument, and to continued *efficient* effort by fresh men, rather than to the long continued manœuvre of one most excellent operator who can no longer command his efforts from fatigue.

In the chapter on ovarian tumors, Dr. Byford contributes a report

of three cases of ovariectomy which he has performed, of which two recovered. One of the chief points of value in Dr. B.'s "Treatise on the Uterus" was the admirable way in which he digested and presented the views of recent authors; but in the present chapter he has taken very strong ground—and remarkably strong for him—regarding the inadvisability of returning ligatures of the pedicle within the abdomen. He states that "this treatment is bad and can not be justified. Perhaps this treatment is fraught with more danger than any other of the ordinary parts of the operation. I regard the dangers here mentioned against the ligature as adding so much to the hazard of the operation that I could not think of making use of it, unless it seemed entirely indispensable in the particular case; * * * * discard scrupulously the ligature and the clamp."

Dr. Byford uses silver pins, which are *passed down to but not through* the peritoneum.

Dr. Byford, however, provides for contingencies in which the clamp may be needed, and for cases where the stump can not be brought out of the wounds. In the latter he recommends a double strong hempen ligature around the peduncle, which is then allowed to drop back into the cavity of the abdomen, leaving the ligature through the lower angle of the wound.

Now we think that many of the points involved in this operation are yet undecided, but we think that the results of Tyler Smith, Clay, Peaslee, Spencer Wells himself, and others, in operations where the ligature has been returned within the abdomen, are not fully set forth in Dr. Byford's book, and that he is not justified in his wholesale condemnation of the practice. It so happens that Spencer Wells' book, and some recent numbers of the *London Medical Times & Gazette*, are on our table at this moment, and a casual reference gives us this result.

In Spencer Wells' case No. CI., "a small pedicle was secured close to the uterus by a silk ligature, which was cut off short and returned." The woman recovered. In case CIII. where, after a similar procedure, the woman died, and the case strongly impressed Dr. Wells with the disadvantage of returning the tied pedicle into the abdomen, we yet find that the "ligatured stump was inclosed by a capsule of lymph."

In the *London Medical Times & Gazette* for July 29, 1865, p. 117, there is a report of a case in which Mr. Wells operated for a multilocular tumor, with a very short pedicle. "An attempt was made to bring it outside with a clamp, but that made such a pull on the uterus that Mr. Wells transfixed the pedicle, tied it doubly with twine, and

returned the stump with the ligature into the abdomen." The patient recovered. We have quoted this last case as illustrative of Mr. Wells' present views, and as showing that he no longer occupies the position given him by Dr. Byford, who, speaking of the time (in 1861) when Dr. Tyler Smith presented his cases to the Obstetrical Society, remarks that "Mr. Spencer Wells very justly reprimanded him for it in a gentlemanly way, by saying that the procedure ought not to be imitated without more discussion." It appears that subsequent "discussion" has impressed Mr. Wells more favorably with the procedure than Dr. Byford.

Moreover, it is not fair that so respected a teacher and author as Dr. Tyler Smith, whose work is so highly prized in this country, should be perpetually represented to the American student as receiving a "gentlemanly reprimand" from Mr. Wells; the more especially when it is most probable that neither Dr. Smith or Mr. Wells ever imagined that the remarks could be interpreted as a "reprimand."

We also think that the experience of the best men is tending towards passing ligatures through the peritoneum so as to bring about prompt adhesion of the peritoneal surfaces, rather than to introduce the ligatures (or pins) down to the peritoneum without transfixion, as recommended by Dr. Byford. And we may express our surprise that Dr. B.'s familiarity with Mr. Brown's practice has not led him to allude to the use of the actual cautery to the stump before returning it to the abdomen.

The space that we have already occupied in the study of this interesting work admonishes us to close, and we regret our inability to allude to the chapter on milk leg and puerperal fever. Taken as a whole, the book seems to us to present evidences of a more hasty preparation than the previous one. We venture to predict that several editions will soon be demanded, and that each one will show the improvements and additions that Dr. Byford is as well fitted to make as any man in this country.

Lectures on Fever, delivered in the Memphis Medical College in 1853-56.

By A. P. MERRILL, M. D., Professor of the Principles and Practice of Medicine. New York: Harper & Brothers, 1865. 12mo. pp. 235.

The author announces as the object of this well printed and attractive looking little volume, to publish the results of his "own long experience in the treatment of Southern fevers;" while at the same time he claims

to have made "a free use of the observations and teachings of others." The subject-matter is sketchily handled, the style is concise and unpretending, and there is much practical matter within a small space. Bacon styles epitomes barren, and they certainly rarely serve to enlarge the views of the student and practitioner, or extend the resources at their command. We can not but regret that Dr. Merrill, with his large reading and abundant opportunity, has not thought fit to revise and enlarge his "Lectures," or even to expand them into a treatise on what he regards "as one of the most difficult subjects of our profession," and "the most common of all diseases which afflict mankind"—Idiopathic Fevers. There are thirteen lectures, or short chapters, two of which treat of Fever in General—fever considered in its abstract relations; three of Intermittent Fever; three of Remittent Fever; one of Yellow Fever; one of Enteric Fever; one of Exanthematous Fevers; and a supplementary one, the thirteenth, containing some "general remarks" on certain diseases exhibiting more or less tendency to periodicity.

We can not conscientiously say that these "Lectures" satisfactorily represent the present state of pathological knowledge of idiopathic fevers; indeed, Dr. Merrill tells us that "their pathology, in all its forms and phases, is involved in obscurity;" and adds, "from the teachings of pathologists you will derive little advantage, except as they relate to local lesions, which, in general, are to be treated as the same lesions are when they appear as primary affections arising from local causes;" from both of which opinions we must express our very decided dissent, and we regard the latter one as mischievous, and calculated to mislead and do harm in practice. Though Dr. Merrill would reject all the innumerable theories of fever, he very candidly says that he has none to offer in their place. While there is much plausibility in the modern phrase of humoralism, he does not think that the doctrine of primary disorder of the blood is supported by either positive or negative evidence; but he inclines, we infer, to the belief, with Virchow and Parkes, that the blood becomes diseased "through the agency of disordered innervation." There are some incongruities in this lecture that we do not care to particularize or discuss.

Since these "Lectures" were written, large and solid advances have been added to our knowledge of the natural history of fevers. Exactness of observation, enlightened investigation, aided by improved methods and instruments of research, and freedom from all theoretic bias, have contributed, within the past decade, to determine, with great certainty, the causes, modes of propagation, development, the

progress and defervescence of fevers. No more valuable means to aid us in a correct and practical study of febrile diseases, their inception, natural course and termination, is the determining of the correlation of temperature with other morbid phenomena.

The sole constant phenomenon of fever, the essential symptom, is an elevation of temperature—the *calor præter naturam* of Galen—which is due to increased activity of tissue change; and there are always, in the presence of fever, two cardinal points demanding special investigation: 1st, to measure accurately the degree of this preternatural heat and fix its significance; and, 2d, to ascertain the amount of the tissue change, estimated by the relative amount of all the excreta to the body weight. It is as incumbent upon the physician in treating a case of fever to measure carefully the degree of heat, and look to the products of the metamorphosis or destruction of tissue, equivalent to the amount of heat in each case, as to count the pulse and the number of respirations and note their relative frequency. Dr. Merrill passes over entirely the thermometry of fever, an omission which no doubt will be repaired in another edition. The observations of Wunderlich of Leipsic, Ringer and Parkes, of London, and Jones, of Augusta, Ga., in this field of labor, have shown us what important results may be arrived at in the matter of the diagnosis and prognosis of fevers. When the typical range of temperature of these diseases is determined, we shall probably have the earliest and best indications of unexpected complications or favorable progress.

In the chapters on Intermittent Fever there is nothing to arrest attention, except it be the pathogenic dogma, "that it is not debility with which you have to contend in ague, but the very reverse, plethora and sthenic congestion," and the natural therapeutic abundance which follows—all the arms of the materia medica being marshaled to subdue what is universally acknowledged to be a self-limited paroxysm. Dr. Merrill, as a natural consequence of his doctrine of sthenic congestion, is inclined to regard blood-letting in the cold stage of ague much more favorably than observation and reading have led us to, and we fully agree with Sir Ranald Martin, that while it has the show of being prompt and energetic, it is, in effect, "haphazard, systemless, operose, and tedious." Nor can we agree with our author when he says, "quinia I consider to be properly a contra-stimulant remedy, its efficacy as an antiperiodic depending, in some measure, upon the degree of sedation it produces," (p. 69,) or when he ranges aconite and arnica in the class of narcotics, (p. 59) The secret of success in the radical treatment of the disease is very truly said to be in keeping up the action of quinia

without interruption; in other words, until the system is thoroughly saturated with the antidotal remedy. The same may be said of arsenic, which should be persevered in, in diminishing doses, until toxical symptoms appear. No special directions are given respecting the treatment of malignant or pernicious agues.

On the subject of Yellow Fever there is nothing which calls for especial notice. The author is of opinion that it differs only from the other forms of periodic fever in this, "that for some unknown reason it has its prominent and characteristic lesion in the stomach." He believes it attributable to the same cause as periodic fever.

The chapter on "Pneumonic Fever" will repay careful perusal. Those on "Enteric Fever" are the most unsatisfactory in the book, and will require, in another edition, recasting to bring them up to the level of our present knowledge of this common and universal form of continued fever, and which Dr. Merrill perversely classes with periodic fever. What can justify such statements as these? "The diagnosis of enteric fever can not of course be made out in its early stages, for the plain reason that its characteristic lesion is not formed, until which it does not differ from other forms of periodic fever to such extent as to enable us to give a confident diagnosis. * * * We know of no symptom in any case which gives us certain assurance that Peyer's glands are especially diseased, until we come to the autopsy. * * * The treatment proper in the early stages need not differ from that adopted in other forms of periodic fever. The anti-periodic treatment will of course be adopted and prosecuted with a vigor proportioned somewhat to the expected violence of the disease. Whenever this is properly applied, and at the proper time, there will be no enteric or typhoid fever at all; for the simple reason that the fever will be arrested and cured before the characteristic local lesions are formed," (pp. 190-1.) Against all which we enter an earnest protest, believing such teachings to lead inevitably to the doing of great mischief. While we do not believe that "carbonate of ammonia, as an excitant, is mostly to be preferred to alcoholic fluids" in the treatment of enteric fever, but think it objectionable on account of irritating the stomach and bowels, we agree with our author that the best food for general use in the course of the disease "is fresh milk, which has been well boiled as soon as taken from the cow," particularly if accompanied by good, pure wine.

We are quite sure that the statement, at p. 23, that "the physicians of Philadelphia have given us pretty much all that is valuable in medical authorship in America," will, in the face of fact and common jus-

tice to the many valuable original monographs, as well as compilations, which have been contributed by members of the profession not resident in Philadelphia, be modified in another edition, to which we would suggest that a table of contents and index be added.

Lectures on Surgical Pathology, delivered at the Royal College of Surgeons of England. By JAMES PAGET, F.R.S., Surgeon Extraordinary to her Majesty, the Queen; Surgeon in Ordinary to H. R. H. the Prince of Wales; Surgeon to St. Bartholomew's and Christ's Hospitals. Revised and Edited by WILLIAM TURNER, M. B., London, F.R.C.S.E., F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh. Third American Edition. Philadelphia: Lindsay & Blakiston. 1865. pp. 737.

A new edition of this standard work of the best surgical mind in Great Britain can not fail to be a matter of interest to the profession in this country, by whom it is so well known and duly valued. The third American is handsomely reprinted from the second and last London edition. Mr. Paget states that, when the time came for a new edition of these Lectures, he "was anxious that they should be revised with all the light of the knowledge of pathology acquired since their publication;" but, although he had collected materials for the purpose, he found himself unfitted for the task of thorough revision, for the reason that, in the active duties of practice, he had had "no sufficient time for either studying or thinking carefully about the many facts and probabilities and guesses at truth which had been added to Pathology." The revision of the work was, therefore, entrusted to his former pupil, Mr. Turner, but Mr. Paget acknowledges to having "so worked with him as to be equally with him responsible."

A careful comparison with former editions will show the general and valuable revision the text has undergone, probably at the hands of the author himself, and there is great wealth of physiological and pathological knowledge of the day in the notes, contributed, it is understood, by the editor.

In the "Lectures," where all the subjects treated of are of vital importance, lying as they do at the very foundation of medical science, and handled so thoroughly and philosophically, it is difficult to indicate any distinguishing excellence of execution; but to us the chapters on tumors have always seemed, in their clearness, thoroughness, and soundness of views, the perfection of medical composition. Recurrent tumors, including "fibroid" and "fibro-nucleated," having characters

which connect them on the one hand with the innocent, and on the other with the malignant tumors, are, in the present edition, properly placed in a group intermediate between these two great divisions—a connecting link between benign and cancerous growths.

We find no modification of the previously expressed views of Mr. Paget on the pathogeny of this class of diseases. He adheres to the division into innocent and malignant, (adding, as we have seen, an intermediate group, named recurrent,) believing these terms, "though not free from objections, to imply a more natural and a less untrue division than any yet invented to replace them," and in this respect differing from Virchow, who denounces this division as altogether unscientific, declaring that the classification of tumors should be based on anatomical characters, not on physiological relations. While Mr. Paget holds that malignant tumors are local manifestations of a disease which already, in its specific material, exists in the blood—as in cancer, a peculiar or cancerous condition of the blood—Virchow, in his recent great work on Tumors,* the complement of his "Cellular Pathology," is inclined to the belief that this *dyscrasia* or *diathesis* is pure hypothesis, and will disappear in a more advanced state of our knowledge; and that even now, in the vast majority of cases, the change in the blood is to be regarded as a secondary phenomenon, due to the absorption of matter from an existing focus, the malignant growth being due to a local irritant. He supports his view by the fact that the greater number of malignant tumors are met with in those parts of the body most exposed to injury, and sustains it by the history of epithelial cancer, as shown in the incrustated warts on the scrotum, face and lip, which the local irritant will develop into the worst form of malignant disease; while our author would explain the true influence of the soot in chimney-sweep's cancer, not to be the determining of the growth of cancer by its continued contact, but by producing "a state of skin which provides an apt locality for epithelial cancer in persons of cancerous diathesis." Whether the cancerous cachexia is primary or secondary, the existence, under any circumstances, of a true blood-cause, and the state of that fluid in cancerous states of the body, are, at this day, with all the lights of modern pathology, and the investigations of Bennett, Lebert, Hannover, Paget, Rokitsansky, Virchow, and others, as much involved in obscurity as they were a century ago. At present we must be content to accept our author's rather unsatisfactory conclusion—"the reconciliation, not only of the two conflicting opin-

* Die Krankhaften Geschwülste. Von Rudolph Virchow.

ions, but of the seemingly conflicting facts upon which they chiefly rest, is to be found in this, that the complete manifestation of cancer, the formation of a cancerous growth, is suspended till such a time as finds both the constitutional and the local conditions coexistent—till the blood and the part are at once appropriate."

Die Ohrenheilkunde der Gegenwart. Berlin: 1860. Von Dr. W. KRAMER. Translated by HENRY POWER, for the New Sydenham Society: 1863.

A former edition of this work, translated in 1837, by Dr. James Risdon Bennett, contained severe criticisms on preceding and contemporaneous writers, each of whom was, in the author's opinion, guilty of neglect or ignorance in reporting or treating cases.

Much that was good in the work was mingled with a great deal that was unpleasant and eccentric.

These characteristics seem to exist to a less extent in the present edition; but Erhard and Toynbee come in for a large share of severe criticism, the former, more especially, as the German expositor of the views of the latter.

The post-mortem researches by Toynbee seem to be considered by our author as of little benefit in arriving at a correct knowledge or treatment of diseases of the ear, because they were, for the most part, made on cases whose history was unknown to Mr. Toynbee. Throughout the work we are strongly urged to study the state of the Eustachian tube on the patient, by means of Kramer's series of five catheters, the "gut" and caoutchouc bougies, and the otoscope or "diagnostic tube."

In ear diseases, as in others, prophylactic treatment is desirable; to this end Dr. Kramer recommends the use of plugs of wool in the ear in cold weather, in mills and other noisy places, and "ear-laps" over those organs in winter, and care not to wash the ears with cold water, all of which precautions, it seems to us, may better be neglected.

Among the symptoms of ear diseases, only two—*hardness of hearing* and *noises in the ears*—are of general signification.

Among 1,000 cases of ear disease taken at large, 496 were accompanied by noise. The cause of these noises, Dr. Kramer says, is irritation of the chorda tympani nerve, produced by the pressure of wax on the drum, or by inflammation of the membrana tympani, or neighboring parts.

The most frequent causes of ear diseases are cold, change of temperature, and exanthematous and catarrhal diseases.

For examining the external ear we are advised to use Kramer's ear speculum (which is valvular, with two long handles and *unpolished* interior) and direct sunlight. The *dullness* of the interior, we think, is an advantage, as thus we are not confused by reflections from the sides of the instrument. The long handles, however, seems to us inconvenient, as they necessitate the employment of both hands while using the instrument, thus rendering it inconvenient to clean out the ear, or perform any operation on it while using the speculum.

For examining the middle ear, Dr. Kramer uses and recommends silver catheters of five different sizes, and gut and caoutchouc bougies. With the aid of these the permeability of the Eustachian tube is diagnosed with certainty. In treatment, caustics may be applied on the ends of the bougies, or fluids may be blown through the catheters into the middle ear.

According to Dr. Kramer, diseases of the drum occur in about one case out of each five of ear disease, he having found them in 218 cases among 1,000.

In discussing perforation of the membrana tympani artificial drums are very slightly spoken of. In our experience they have frequently proved beneficial. Patients often can not wear them constantly, but for a lecture or some amusement of an hour they may materially improve the hearing. They should be used only when the Eustachian tube is pervious.

Inflammation of the middle ear is very often mistaken for nervous deafness. Dr. Kramer divides it into inflammation with (*a*) suppressed, (*b*) free, (*c*) free and interstitial, and (*d*) exclusively interstitial exudation.

Catarrhal inflammation with exclusively interstitial exudation is the most frequent of ear diseases, forming about 40 per cent. of all cases. The symptoms are slowly increasing deafness, thickening of the drum, (by interstitial exudation,) redness and swelling of the fauces and tonsils. This is the disease called by Tröltsch "Chronic Catarrhal Inflammation of the Middle Ear." The treatment recommended by Dr. Kramer is the application of astringents to the throat and Eustachian tube.

Of nervous diseases of the ear we have but little positive knowledge. They come from falls, explosions, apoplexy, fevers, etc. Unless the pathological cause of the disease is known, we can do but little in

the way of treatment. If the affection probably depends on congestion, we may use leeches and resorbents.

Under the head of Diseases of the Internal Ear, (that is, of the bony and membranous labyrinth,) are placed acute and chronic inflammation of the labyrinth, acute inflammation of the facial nerve within the fallopian canal, nervous deafness, and deaf-mutism.

Acute inflammation of the labyrinth is generally caused by injury from some instrument introduced through the external ear. Its symptoms are pain in the temporal region and deep in the head, drowsiness, vomiting, etc. It is to be treated by cool applications, purgatives, and antiphlogistics generally.

Deaf-mutism is "congenital" and "developed." The distinction of "early" and "late development" is not used by Dr. Kramer. He says that it is generally congenital, and although not hereditary, the parents are loth to acknowledge that it has existed from birth.

The successful use of electricity or galvanism in these cases by Bamberger, Duchenne and Blanchet, as well as that of ear-trumpets by Toynbee, is much doubted by Dr. Kramer.

Dr. Kramer is one of the most noted and successful of the aural surgeons of the present day, and his work would perhaps have been more interesting and useful to the profession, if it contained at greater length *his own ideas* of pathology and practice, and less extensive notices of faults (supposed or actual) of other writers on aural surgery.

The Science and Practice of Medicine. By WILLIAM AITKEN, M.D., Edin. Professor of Pathology in the Army Medical School, &c., &c. In two volumes; third edition; revised and portions re-written. London, 1864; 8vo., pp. 939, 993.

This work first appeared as a Hand-Book on the Practice of Medicine, written for the Encyclopædia Metropolitana; it met with a ready recognition of its excellence by the profession, and a rapid sale. The second edition was almost entirely re-written and expanded into two thick volumes, and, so favorably was this received, that within six months of its publication it was exhausted and a third called for.

Dr. Aitken is the Professor of Pathology in the Army Medical School at Netley, and during the Crimean war was attached, as Pathologist, to the military hospital of the British troops at Scutari.

We have in this work a view of the main facts, doctrines, and practice of Medicine in accordance with its present state. It is an admirable compendium for reference or study, being a digest of all the recent

treatises and monographs, and embodying the latest discoveries and improvements, by a practical man of large opportunity. The knowledge of the day on the science and practice of medicine is summarized with care, exactness, lucidity, breadth, and sound judgment.

The department of Medical or Noso-Geography, which treats of the conditions by which health and disease are distributed over the globe, and constitutes the basis of hygiology, receives attention for the first time in a manual on practice of medicine. It is a valuable and instructive chapter, revised by Dr. T. G. Balfour, Chief of the Statistical Bureau of the British Army Medical Department. The practical importance of a study of the temperature or thermometry of disease is insisted on, with an analysis of the observations of Wunderlich, Traube, Ringer and Parkes. Indeed we suspect that for the prominence given in these volumes to this important but neglected means of diagnosis and prognosis, we are indebted to the author's colleague at Netley, Dr. E. A. Parkes, who, as Professor of Clinical Medicine in University Medical College, devoted so much time to the teaching of this important topic.

We can commend Dr. Aitken's Science and Practice of Medicine most heartily to the profession, and as deserving of a place in every medical library.

CORRESPONDENCE.

THE EPIDEMIC CHOLERA OF 1849 AND 1854.

Editor of N. Y. Medical Journal:

"The medical men here are very reticent in giving opinions on the cholera."

The above is quoted from a New York daily paper of the 4th inst., and the statement is, perhaps, too true. Every man should now speak out and tell us what he knows about cholera. By comparing opinions, or rather by comparing observations and experience, we may arrive at some practical result.

If cholera shall visit us, we must be ready to meet it, and do all we can to stay its ravages.

I beg to give you, in a hurried manner, the result of my observations during the epidemics of 1849 and 1854. The mortality in 1849 was great, and perhaps great because of much bad treatment. The disease was considered desperate, and desperate remedies were admin-

istered. Opium, calomel, quinine, sugar of lead, strychnine, etc., were given alternately, or all together, as the ambitious doctor thought best.

The scourge passed away, and we knew about as little of its cause or its treatment as we did before it came. In 1854 the disease was treated with better success. Perhaps the epidemic was not as severe as in 1849.

The city of Williamsburg (now the Eastern District of Brooklyn) organized a Board of Health, and established a Cholera Hospital, July 13th. I visited my first case of cholera that year, and up to July 29 I had treated five other cases. July 30 the Cholera Hospital was opened, and during August, twenty-six cases were admitted, and in September, nine—making, in all, thirty-five cases. There were twelve females and twenty-three males—twenty-nine over 20 years of age, and six under 20 years of age; seventeen born in Germany, fourteen in Ireland, three in England, and one in New York. A large portion came into the hospital in an advanced stage of the disease, and some in complete collapse. Two men died of brain disease. They had no cholera discharges after they came in.

One woman was received with a babe but two weeks old, and both died. Another woman was convalescing from cholera, and died of exhaustion from uterine hemorrhage.

The cases sent to the hospital were of the most destitute class, who could not be cared for at home, and most of them had been largely *drugged* before they came in. Some were drunk with bad whiskey, and some were drunk with opium. From the class and character of the patients, it will be readily inferred that most of them died. And such was the fact. Only fifteen out of the thirty-five recovered. Those that recovered were under treatment from six to twenty days. Those that died were under treatment but a few days, and in some cases but a few hours. Five or six died soon after admission, and were not treated at all. The last patient was discharged from the hospital September 9th.

Cholera made its appearance in Williamsburg suddenly. Nine cases occurred about the middle of July, within two or three days, in one block, and in a respectable locality. From that time isolated cases were found all over the city, more particularly in the filthy districts, and where the people were huddled together. About the middle of August, eight cases occurred in one tenement house. The lower part of this house was a filthy shoe-shop in front, and a kitchen back, and the whole family, four in number, died. Tenants up stairs took the disease and died. The Sanitary Commission visited the spot, ordered

the dead to be buried, sent the sick and dying to the hospital, took possession of the house, limed it, and shut it up. This at once checked the spread of the disease in that locality. Yet it is a fact worthy of mention that four other cases can be traced to that house. One was a sister to the shoemaker, who came from the country to help take care of the sick. She returned to her home in Astoria, where she was at service, and in a day or two sickened with cholera and died.

Two others, who lived in a small house on the rear of this lot, took the disease and died. A washerwoman, who washed the bed-clothes of the shoemaker's family, in the yard of these premises, went to her home in another part of the city, and died of cholera in a few days. Do these facts help to prove contagion?

A few *autopsies* were made, and they were made in great haste. In those that died in the algid stage, there were no perceptible traces of disease, except a more or less injection of the mucous membrane of the whole alimentary canal. In those that died some days after reaction, and with more or less of fever, there were unmistakable evidences of congestion and inflammation of the solitary and aggregated glands of the colon, lesions similar to those of typhoid fever. The elliptical plates were elevated, and slight patches of ulceration were seen extending through the mucous membrane. My notes do not remind me of the exact number of autopsies made, and I can only recollect five or six. My private practice was crowding me, and my work was imperfectly done.

The treatment of such cases as could be treated at all was entirely experimental. Astringents, tonics, and stimulants were given without limit, but the vomiting was generally incessant, and the remedies rejected by the stomach. Medicines were administered by injection only to be pushed away in a few minutes by a large watery passage. I observed that when brandy was thrown into the rectum, the next stool would be less in quantity. I then ordered brandy and strong coffee injection to one patient, brandy and a solution of sugar of lead to another, and brandy and a solution of tannin to another, and found they were all retained a considerable time, and each passage that followed was diminished in quantity. These injections were repeated after each passage, and each time the stools were considerably postponed and smaller, and in a little time the bowels were checked entirely. I then ordered brandy and strong tea, and afterwards brandy and water, by injection, with the same result, and found that *brandy* was the astringent and stimulant that controlled the watery stools. Patients would be brought in approaching collapse. They had passed

much fluid by the bowels, and were continually having large watery stools every few minutes. They were in a cold and clammy sweat; the skin blue and shriveled, the tongue cold, the voice husky, rolling and tossing in bed, and screaming with cramps in the legs. The nurses would take them by force, and give them injections of two or three ounces of brandy, and as much strong coffee or tea, and repeat the injection immediately after each passage; and, usually, three or four injections would control the passages, reaction would come on, the pulse would increase in volume, the surface of the body would become warm, the skin assume a more natural hue; in short, all the vital functions improve, and the patient go on to convalescence. After reaction had been established, small doses of calomel and Dover's powder were given to act upon the liver, and also to relieve the colic or pain in the bowels that usually follows the sudden check of the cholera stools. If the colic was severe, opium was given in full doses. In a day or so castor-oil was given to carry off the calomel, and produce bilious stools.

The distressing cramps in the extremities were most relieved by dry rubbing with coarse flannel to get up capillary circulation; hot mustard plasters placed over the stomach and liver are of great value in arresting the vomiting so persistent in the early stages of cholera.

Mustard may stimulate the action of the liver, which is always desirable. It will stimulate the skin, and did very much in bringing on reaction.

The loss of a number of gallons of fluid by the bowels is a direct drain upon the blood, and the prostration that follows almost as great as from a true hemorrhage. The circulating fluid is so much reduced in quantity that it does not fill the blood-vessels. Much of the serous portion has leaked away, leaving the solids of the blood in the cavity of the circulation, and too thick to circulate. Hence we have cramps in the extremities, because there is not circulating fluid enough to go the complete rounds of the circulation, and the blood too thick to find its way into the smaller blood-vessels.

The blood is the natural stimulus of the body, and when any parts of the body are deprived of their stimulus, the nerves of those parts are not kept under healthful or physiological control, and we have muscular contractions and muscular spasms.

Opium given in large doses to relieve these cramps is positively hurtful. It will relieve them only by narcotizing the patient and destroying sensibility to pain.

I am fully satisfied that much mischief has been done by opium in the treatment of cholera. It may be given in stimulant doses, combined with strong coffee, to assist in bringing on reaction, watching carefully its effects upon the brain. Hot lemonade and brandy, and hot coffee and brandy in small quantities given by the stomach will aid very much in hastening reaction, and in supporting the exhausted patient.

O. H. SMITH, M.D.

NEW YORK, Nov. 13, 1865.

PROGRESS OF THE MEDICAL SCIENCES.

I.—ANATOMY AND PHYSIOLOGY.

The following abstracts of papers read before the late meeting of the "British Association for the Advancement of Science," will, doubtless, prove interesting to our readers.

1. *On the Effects of Scanty and Deficient Food.* DR. DAVY.

The author had never found any instance where, in the adult man, a deficiency of food had, though causing temporary weakness, produced any permanent ill effects. None of the Arctic or African travelers had suffered any permanent injury through shortness of food. There were some in whom he thought fasting was beneficial, and even sound healthy men might be benefited by an occasional long fast. He supported the view of a somewhat scanty prison diet, to keep the criminal in a state of healthy activity somewhat below par. Excess of diet in jails tended to increase the prison population, as vagrants and others were in the habit of looking upon it rather as a place of refuge and comfort in winter and bad weather. He urged an inquiry to determine the lowest scale of diet that could be used in prisons, and that should not injure the constitution. Dr. Bennett (of Edinburgh,) Dr. Edward Smith, Dr. Rolleston and others, discussed the subject. There was great diversity of opinion upon the general question of prison diet, but in the main it was opposed to the views of the author, and some of his assertions were negatived.—*Lancet*.

2. *On the Prevalence of Tapeworm in Birmingham.* DR. FLEMING.

As compared with Edinburgh and London, the greater frequency of the disease in Birmingham was very much marked, and this was proved by statistics. Dr. Fleming considered the chief cause of this to be measly pork, especially in the form of sausages, largely eaten imperfectly cooked by the poorer classes. As a preventive he advised that the pigs should be supplied with thoroughly clean food and drink, and kept as far as possible from dogs; and that the official

inspection of living and dead pigs should be made as searching as possible. Dr. Crisp had heard that in Germany cooks were most affected with tania, and he thought the immunity from it in Edinburgh was due to the use of oatmeal as a diet. Dr. Cobbold said that beef and veal were equally sources of it, and that many persons were affected with no outward signs of it. Mr. Hughes and Prof. Bennett also made some remarks, and the President thanked Dr. Fleming for his excellent paper.—*Lancet*.

3. *Rigor Mortis not Muscular Contraction.* Dr. NORRIS.

The author said the commonly received theory is, that it is an energetic muscular contraction, which is erroneous, as proved by the following: 1. The rigor of opposing sets of muscles does not cause the redispotion of limbs in obedience to the superior powers of the stronger sets of opponents. 2. It does not cause the rupture of the weaker set of opponents. 3. If either of the flexors or the extensors of a limb be divided, and the limb be placed, before rigor has set in, in the position in which it should be drawn by the cut set of muscles, and the action of these been unopposed, the uncut set of muscles do not alter that position. 4. Contraction and the presence of irritability being an inseparable association, it follows that if irritability be absent for a long time, immediately prior to the supervention of rigor mortis, the latter can not be regarded as a contraction. 5. The microscopical appearance of muscular tissue affected with rigor mortis is entirely different from that of muscular tissue in a state of contraction. The truth of these arguments he had demonstrated by a variety of experiments, selections from which were set forth in detail. Photographs of some of his results and examples of others were submitted to the section. So highly important were these considered that the committee recommended him to continue his experiments and observations, and a grant of money was awarded to enable him to carry them out.—*Lancet*.

4. *Influences of Civilization on the Brain.* Mr. ROBERT DUNN.

The following formed the basis of this paper: 1. That the brain or encephalon is the material organ of the mind. 2. That there exists a close correspondence in form and size between the cerebrum and its outward bony covering, the skull; so that the varying forms of the human cranium indicate, as outward and visible signs, with certain well-understood qualifications, corresponding differences or changes in the shape and size of the cerebral substance within. 3. That the *genus homo* is one; and that all the races of the great family of man are endowed with the same intuitions, sensational, perceptive, and intellectual—the same mutual activities, however they may differ in degree; and that they all have the essential constituent elements in common of a moral, religious, and intellectual nature. In the discussion Dr. Hinds, Dr. Scott, Mr. Vivian, Lord Milton, Dr. Child, Mr. Faulkner, Dr. Camps, and others took part. Of course a wide difference of opinion prevailed amongst the speakers. Viscount Milton contended that the action of the brain, like that of any bodily organ, attracted to it an increased flow of blood, and necessarily led to an enlargement of bulk as regarded the nutritive process, and an increased vigor of function.—*Lancet*.

5. *On Variability, as Manifested in the Construction of the Human Body.*

WILLIAM TURNER, M.B., F.R.S.E.

On this subject Mr. Turner read a very able and interesting paper. He commenced by stating that as there was a considerable amount of variability of the human frame, it was no matter of surprise that differences and variations should exist in parts of the body not outwardly visible. The internal structural variations were so numerous that he would be compelled to confine himself almost entirely to some variations he had observed in the hands and feet. There were many variations not outwardly visible, and which required careful study and observation. He proceeded to notice the variations of the bands of the thumb and fingers. These, in the observations he had made, were very variable, and by aid of numerous diagrams he pointed out the many differences in the construction of the muscles of the hand. The various diversities in the subdivisions of the muscles of the hand were ably and clearly explained and illustrated, and Mr. Turner then proceeded to notice the variations in the flexor muscles of the toes. He gave his experience of fifty examinations, and remarked upon the various changes that had come under his observation. Mr. Turner made some remarks upon the different opinions and statements that had been offered respecting the anatomy of the toes and hands, and proceeded to observe that these opinions were right as far as they went; but having had opportunities of studying the subject of the paper extensively, and making many and frequent observations, he was enabled to go further into the subject, to add something, he hoped, to what had already been written and said on the subject. He concluded by noticing that the variations he had alluded to were as numerous as they were peculiar; in fact, that neither form nor structure were stereotyped; but how far these variations affected the general condition, and whether they were transmitted from parents to children, were subjects he could not enter into. In the development of each individual, a morphological specialization occurs, both in internal structure and external form, by which distinctive characters are conferred, so that each man's structural individuality is an expression of the sum of the individual variation of all the constituent parts of his frame. The illustrations advanced in support of the author's opinions were taken from the flexor muscles of the fingers and toes, from the modifications in the form and size of a foramen, called supracondyloid, which is occasionally met with in man, and of the objects passing through it. It was also shown that variability in construction was not manifested merely in different individuals, but that in the same individual the corresponding structures on opposite sides of the body were by no means symmetrically disposed.—*Med. Times and Gazette.*

6. *On the Relative Weight of the Brain in relation to the Intelligence in the Vertebrata.* DR. CRISP.

Dr. Crisp illustrated his paper with a large number, 126, of plaster of Paris casts and drawings, and commenced by dividing the subject into three principal heads, which were afterwards subdivided into many more. He said that in the brains of mammalia there was great uniformity, but in fishes the brain was very irregular. He thought much error prevailed with respect to intelligence being regulated by the number of convolutions in the brain, for it was found that animals

with few convolutions were very sagacious, while in other classes there were a great many very stupid. The brain always remained of the same weight, though the body might decrease or be augmented in bulk, and the brain of the smaller animals was relatively greater than that of the larger. The brain of a man weighed about thirty-eight ounces, and that of a woman four or five ounces less, forming a proportion of 1-35th to 1-40th of the weight of the body. After entering into comparisons of weight of brain, illustrated by drawings and models, he said his researches had led him to the following conclusions: The external appearance of the brain in most animals was, to a great extent, an indication of the comparative amount of the intelligence of the animal; but to this rule there were many exceptions, as, for example, in the elephant, dog, seal, and many birds. The same inference might be drawn from the covered and uncovered state of the cerebellum, as instanced in the animals last named. The number of convolutions was to some extent a tolerable indication of the amount of intelligence in mammals; but to that there many exceptions. Looking to the weight of brain in 226 animals which he had selected, he found that the relative weight of brain in proportion to the body was an indication of intelligence, for he had ascertained that there was no example of any animal with a relatively small brain that possessed a great amount of intelligence.—*Med. Times and Gazette.*

7. *Ozone.* Dr. B. W. RICHARDSON.

The following are the reliable facts known up to this time respecting ozone: 1. Ozone in a natural state is always present in the air in minute proportions; viz., one part in ten thousand. 2. It is destroyed in large towns, and with special rapidity in crowded, close, and filthy localities. 3. Ozone gives to oxygen properties which enables it to support life. In this respect it acts like heat; its effects are destroyed by great heat. 4. Ozone diffused through air in minute quantities produces, on inhalation, distinct symptoms of acute catarrh. 5. When animals are subjected to ozone in large quantities, the symptoms produced, at a temperature of 75°, are those of inflammation of the throat and mucous membranes generally, and at last congestive bronchitis, which, in carnivorous animals, is often rapidly fatal. 6. When animals are subjected for a long period to ozone in small proportions, the agent acts differently, according to the animal. The carnivora die, after some hours, from disorganization of the blood separation; but the herbivora will live for weeks, and will suffer from no acute disease. 7. The question whether the presence of ozone in the air can produce actual disease, must be answered cautiously. Science has yet no actual *demonstrative* evidence on the point. But the facts approach to demonstration that catarrh is induced by this agent. All else is as yet speculative. 8. During periods of intense heat of weather the ozone loses its active power. 9. On dead organic matter undergoing putrefaction, ozone acts rapidly; it entirely deodorizes by breaking up the ammoniacal products of decomposition. At the same time it hastens the organic destruction. 10. There is an opposite condition of air in which the oxygen is rendered negative in its action, as compared with the air when it is charged with ozone. Air can thus be rendered negative by merely subjecting it, over and over again, to animals for respiration. The purification of such air from carbonic acid and other tangible impurities, does not render it capable of supporting healthy life; but ozone restores the power.

In a negative condition of air the purification of the organic matter is greatly modified, and the offensive products are increased. Wounds become unhealthy and heal slowly in such negative air. 11. There is no demonstrative evidence, as yet, that any diseases are actually caused by this negative condition of air; but the inference is fair that diseases which show a putrefactive tendency are influenced injuriously by a negative condition of the oxygen of the air. It is also probable that during this state decomposing organic poisonous matters become more injurious. 12. As ozone is used up in crowded localities, and as it is essential that ozone should be constantly supplied in order to sustain the removal of decomposing substances and their products, no mere attention to ventilation and other mechanical measures of a sanitary kind can be fully effective, unless the air introduced be made active by ozone. Fever hospitals and other large buildings in towns should be artificially fed with ozonized air.—*Brit. Med. Journal*.

8. *On the Functions of the Cerebellum.* Dr. W. H. DICKINSON.

This paper was founded partly upon experiments made on a great variety of animals, chiefly of the lower order, and partly upon observations on human pathology. The general results were as follow: 1. The addition of the cerebellum to the medulla oblongata gives an increase of voluntary motive power to the four limbs—to the posterior in a greater degree than to the anterior. The power thus obtained is distributed in such a way as to produce even and balanced movements, and often appears to be exercised in a continuous and automatic manner. 2. The removal of the cerebellum has an effect upon the muscles of the limbs, which increases in proportion as the organ increases in size. It consists in a diminution of voluntary power and of muscular adjustment. When an inequality of effect can be noticed, the loss is greater in the posterior limbs. There is a loss of habitual activity. From the effect of lateral injuries, it must be assumed that each lateral half of the organ has an influence on both sides of the body, but to a greater extent upon that opposite to itself. 3. The removal of the cerebellum has no effect upon superficial sensation, on any special sense, on the action of the voluntary muscles, or on reflex movements. 4. In the human being it appears there is no constant effect from loss or alteration of the cerebellum, but failure of voluntary muscular power. Disease, or deficiency of the whole organ, invariably lessens voluntary power in the limbs, especially in the lower. The loss of one lobe produces its effect more on the opposite side than on its own. Disease confined to the cerebellum has no effect upon superficial sensation, on the intellectual powers, or on the action of muscles supplied by the cranial nerves. Hence it appears that the function of the cerebellum is to supply the voluntary muscles of the trunk and limbs with self-regulating motive power. This is distributed in an inverse manner to the influence of the cerebrum. The latter has the sole control over the parts supplied by the cranial nerves, and the chief control over the anterior limbs. The cerebellum has its greatest effect upon the posterior limbs, less upon the anterior. Thus the muscles of the trunk and limbs are under a double rule, while those of the head and neck are regulated solely by the cerebrum. It appears that cerebellar movements are apt to be continuous and habitual, contrasting with the emotional character of those which originate in the cerebrum.—*Brit. Med. Journal*.

9. *Experiments confirmatory of those of Kühne, on the Non-existence of Ammonia in Blood.* Dr. A. GAMGEE.

Few questions had excited greater discussion among physiologists than the coagulation of the blood, and few researches on the subject had been received with greater interest by the scientific world than that of Dr. B. W. Richardson. The conclusions Dr. Richardson had come to were, that the blood, whilst circulating in the living body, contained free ammonia; that when the blood leaves the animal body, ammonia escapes and coagulation takes place, the escape of the ammonia and the phenomenon of coagulation being considered to stand in the relation of cause and effect. He (Dr. Gamgee) made some experiments which were in direct opposition to those of Dr. Richardson; and as the quantity of blood which Kühne and Strauch subjected to analysis was comparatively small, he determined on repeating the experiments in the most rigid manner possible, and upon a larger scale than had been previously attempted. Having described these experiments, Dr. Gamgee concluded by remarking that further researches would, he believed, confirm the results of the experiments which he had performed, and satisfactorily prove that Nersler's reagent was as delicate a test for the compound ammonia as for simple ammonia.

A discussion followed the reading of the paper, in the course of which Dr. Richardson controverted the views of Dr. Gamgee.—*Brit. Med. Journal*.

10. *Cell-Pathology.* Dr. J. HUGHES BENNETT, F.R.S.E.

Dr. Bennett stated that a cell-pathology had naturally sprung from the cell-theory, as originally framed by its founders, Schleiden and Schwann, which had greatly extended the boundaries of medical science. The cell-pathology of Virchow, however, was based upon a law he sought to establish, viz., that every cell sprang from a pre-existing cell, and that we must not transfer the seat of oval action to any point beyond the cell. This supposed law, he maintained, was opposed by so many histological facts as to be altogether untenable. He begged especially to draw attention to the origin of puss-cells, which Virchow and some of his followers had represented as originating in the interior of connective tissue corpuscles. Dr. Bennett and his pupils had frequently sought, by passing setons through the skin and muscles of animals, to observe in the inflamed tissues the appearances which had been figured in support of Virchow's views, but had never succeeded in seeing pus-cells within pre-existing cells. Henle had pointed out that the error had originated in mistaking the triangular spaces observable, on a transverse section, between the bundles of various fibrous tissues, for cells; as in these, unquestionably, pus was very likely to collect. Dr. Bennett further believed that the tendency of many cells to enlarge as the result of irritation, and to multiply themselves endogenously, as shown by himself, by Roberts, Goodsir, Redfern, and other pathologists, was another source of mistake among the younger histologists. The granules and included cells so formed were mistaken by them for those of pus, though easily separated from them. He called attention to a series of preparations (which were exhibited,) showing suppuration in inflamed eye-balls, and in pneu-monic lungs, in which pus-cells might be seen in all stages of formation—originating from a coagulated molecular exudation, unconnected with any pre existing cells whatever. In the sections of lung more

especially, the fibrous tissue of the organ surrounding the air-cells might be seen to be quite healthy. In the coagulated exudation, on the other hand, the molecules might be observed at first uniformly filling up the air-vesicle; then formed into masses, varying in size from the twenty-thousandth to the one-thousandth of an inch in diameter. The latter were rounded, and were identical with pus-corpuscles. He believed that these bodies, therefore, were formed by an aggregation of smaller particles or molecules, composed originally of the coagulated exudation. It was certain that, in the situations referred to, they did not originate in pre-existing cells, as no such cells could be seen. If, as might be supposed, they sprang from the epithelial cells lining the chambers of the eye or the air-vesicles, such cells would be seen, enlarged, and containing the pus-bodies. But his preparations and numerous examinations of the part when diseased had proved to him that no such cells were mixed with the exudation, or in any way connected with the formation of pus.—*Brit. Med. Journal*.

11. *Is the Opinion that a Diet of Animal Food conduces to Leanness well Founded on Facts?* Dr. JOHN DAVY, F.R.S.

Dr. Davy said those who have advocated the opinion that a diet of animal food conduces to leanness, had supported it by arguing that a vegetable diet was commonly richer than flesh in the elements from which adipose matter is formed, such as starch, &c., and further, that carnivorous animals were commonly leaner than herbivorous. He disregarded the first argument, inasmuch as certain kinds of animal food abounded in fatty matter. He instanced the case of animals subsisting on other animals, all of which were very fat, and he considered that tended to show that a diet of exclusively animal food was in no wise incompatible with fatness. Referring to our own species, it was easy to find corroborative instances. Butchers and their families, who used large quantities of meat, were not remarkable for leanness; and fishermen and their families were generally stout. The English, as a rule, had always been considered large consumers of meat, especially in the olden time, when vegetables were less abundant; and in those periods they were notorious for their stoutness. Did a vegetable diet tend to the production of fat? The Irish, living mostly on potatoes, should be distinguished for lustiness, though they certainly were not; and he had not heard fatness ascribed to vegetarians. Amongst our soldiers and sailors a fat man was a rarity; but that was no wonder, for though their diet contained a large proportion of animal matter, their meat ration was never in excess, and they were rather underfed than overfed; while at the same time they had a great deal of exercise. His opinion, in which he was supported by eminent physiologists, was that a mixed diet, partly animal, partly vegetable, was best adapted to the wants of man, as well as most suitable to his taste; and that the safest way to avoid obesity was to live moderately, observing the happy medium between a too sparing and a too copious dietary; and, for the correction of obesity, attending rather to quantity than quality of food.—*Dublin Med. Press*.

12. *Physiological Action of Carbonic Acid.*

M. Demarquay, in a memoir presented to the Academy of Sciences, gives the following conclusions: 1. Carbonic acid has an irritant action on the skin, especially in the more secretive parts, such as the perineal region. 2. Analgesia of the skin is obtained only by the influence of a continuous jet of the gas on a very limited part of the body. 3. The action on the organs of sense is analogous to that on the external integument; there are lively excitement, sensorial exaltation, or nervous perturbation—all these phenomena being generally very transient. 4. The action on the digestive organs is stimulant, and is attended by slight neuro-vascular excitement. 5. Injected into the veins, carbonic acid is absorbed in large quantity, and is rapidly eliminated if the experiment be carefully performed; otherwise it produces distension of the cardiac cavities, and death. 6. Carbonic acid introduced into the air-passages does not produce the toxic effects commonly attributed to it. Mammalia can remain for some time without serious inconvenience in an atmosphere of common air or oxygen mixed with from 20 to 25 per cent. of carbonic acid. In man, some slight disturbance occurs at a time varying with the susceptibility of the individual. When death follows the inhalation of this gas, whether in man or in animals, the *post mortem* appearances are different from those produced by carbonic oxide, the effects of which have often been confounded with those of carbonic acid. 7. Most of the accidents produced by the fumes of charcoal, by confined air, by the vapors arising from stoves or from fermentation, have been erroneously attributed to carbonic acid; they ought in great part to be laid to the charge of carbonic oxide, sulphide of hydrogen, alcoholic vapor, and other imperfectly known gases. 8. Carbonic acid is simply irrespirable; because, being of the same nature as the gas expired from the lung, a physical interchange of gases can not take place. Hydrogen and nitrogen are less immediate in arresting respiration than carbonic acid, because they allow the physical interchange to go on for a short time. 9. Anæsthesia can not be produced in man by carbonic acid without danger of asphyxia; and, even if it could be safely produced, it would be too transient to be of service in operations.—*Gaz. Méd. de Paris.*

II.—SURGERY.

1. *Two Tumors removed from the Larynx in a Case of Aphonia of Six Years, followed by immediate Speech.* DR. GIBB.

The patient was a single lady, of thirty-eight, who had had a bad throat for six years, the voice being reduced to a croupy whisper. During the first three years she had coughed up pieces of "flesh," one of which was an inch long and the shape of a shrimp. For the greater part of the time she could not lie down at night, from supposed cardiac disease with dyspnoea, and her complexion was very florid. On examination with the laryngoscope in April last, a long, fleshy, somewhat bulbous growth was seen to occupy the greater part of the sub-glottic space, springing from the anterior part of the larynx, below the origin of the true vocal cords, and quite immovable. The larynx in other respects was comparatively healthy, but there was

much irritability and spasm of its proper muscles. After six weeks' preparation, Dr. Gibb snared a growth in the loop of wire of his laryngeal *éraseur*, cut its pedicle, and withdrew it firmly held by a piece of uncut mucous membrane in the retracted loop, in a similar manner to the outer coat of the artery after ligature. He now found a second and larger growth, which had formed the bed of that already removed, and this was snared six days later in a similar manner to the first, the tumor being likewise withdrawn in the retracted wire loop of the *éraseur*. The voice was immediately restored, for the mechanical obstruction was got rid of. In a few days the little wound cicatrized, no obstruction was visible, the trachea was normal, and the patient left for the North—cured. The composition of the tumors was wholly epithelial cells, and to the naked eye resembled a congeries of small cysts; they were of the size of small beans. The tumors were exhibited with the wires attached to the mucous membrane, and Dr. Gibb remarked that when the loop of the wire was not violently or spasmodically drawn home, the growths invariably were withdrawn with the instrument itself. The case made, he believed, his fifteenth or sixteenth in which he had now successfully abstracted growths from the larynx by means of wire loops, and this in their entirety, not in fragments. The shape and position of the tumors in the present instance were well illustrated by a large diagram.—*Medical Circular*.

2. *Cases of Erysipelatous Inflammation of the Scrotum and Penis, Simulating Extravasation of Urine, with Remarks.* Communicated by F. HOWARD MARSH, late House Surgeon to St. Bartholomew's Hospital.

The following cases bring to light no fact which has not been already observed and recorded. They are, notwithstanding, a valuable group, exemplifying as they do the main features of a rare and dangerous malady, which is rendered the more important by the liability which exists that those who have to deal with it may fail at first to recognize its true nature, and mistake it for extravasation of urine, to which it presents in many of its symptoms so close a likeness. The affection, which consists of erysipelatous inflammation of the tissues of the scrotum, generally involving those also of the penis, and spreading more or less widely to the parts around, may arise either as a primary disease, met with as such chiefly in persons above middle age, whose health is reduced in many instances by advanced kidney disease, or from some local condition, as in Case 2, in which the original mischief was deep-seated abscess in the perineum. It is described under the name of "inflammatory oedema" by Mr. Liston,* who says of it that "it often follows on sores, or eruptions situated in the groin, genitals, or inside of the thighs, or on fistulae about the perineum and anus." The mischief spreads rapidly "from the infiltration, apparently of a very acrid and fetid sanies." This writer adds that he had in the Edinburgh Hospital "no less than six cases under treatment at one time, in a very unhealthy season, and in different stages of their progress." Mr. Holmes† has reported some well marked examples of the disease, and, in some remarks which he has appended to them, he dwells on the important point that the retention of urine which is

* "Practical Surgery," 1846, p. 450.

† *British Medical Journal*, 1855-1856.

liable to occur, as was the case with the patient under Mr. Paget's care, and also with the boy in the Children's Hospital, and which is so likely to divert attention from the true nature of the case, is the result, and not the cause, of the mischief in the scrotum.

Although the first step in the treatment of this affection is the same that is proper in the management of extravasation of urine—namely, to relieve the tissues of the acrid fluid by which they are infiltrated, by making sufficient incisions for its escape, there is one consideration which, besides the love of a right diagnosis for its own sake, makes it highly important to recognize this affection in its early stage. In extravasation of urine many surgeons deem it proper that a catheter should be kept in the bladder for a certain period. There is, of course, no necessity for this measure in the disease under notice, and to avoid it is of the first moment, when the patient is a debilitated person who, as experience shows, may be supposed to be the subject of disease of the kidneys. Two circumstances, in the majority of instances, will lead to a correct conclusion as to the nature of the disease. There will, in the first place, be no history of previous obstruction, or of retention of urine; and, secondly, a large catheter will pass into the bladder without encountering any obstruction. The absence or presence of fullness in the perineum is not a safe guide; mere erysipelatous inflammation may have its origin there, as in Mr. Paget's second case, and so resemble in one of its chief features ordinary extravasation of urine; or, on the contrary, extravasation may occur from ulceration of the urethral wall behind a stricture situated in the anterior portion of the urethra, in which case the perineum will be at first in a natural condition, and will only become involved as the extravasation spreads from the penis to the parts around.

CASE I.—*Erysipelatous Inflammation of the Scrotum and Penis, Simulating Extravasation of Urine.* Under the care of Mr. PAGET. J. B., 49, a laborer, poorly off, and in the habit of drinking freely, came to the surgery on September 20. He was in a state of such great prostration that he had scarcely been able to reach the hospital, and on his arrival was at once sent into one of Mr. Paget's wards. Upon examination, the scrotum and penis were found greatly swollen, the scrotum being about as big as the head of a child four years old; the skin of it and of the penis was for the most part of a dull, ash-gray color, but here and there were greenish-black patches, where gangrene was already complete. Some slight brown discoloration of the integuments was seen creeping up over Poupart's ligament. The perineum looked in a natural condition. The patient's general state was that of great exhaustion; he looked pale and depressed; respiration was rapid, and the pulse quick, small and very feeble. He was very confused in the account he gave of himself; but it was gathered that he had never had any difficulty in micturition till the early part of the previous day, (September 19.) The condition of the scrotum and penis had commenced as a painful swelling, which had spread from the lower part of the scrotum, and which he first noticed about two days before his admission, (about the morning of September 18;) but of this he was not clear.

Before proceeding to any treatment, Mr. Paget remarked that, at first sight, the case presented the ordinary features of extravasation of urine; yet he believed it would prove that the patient was not suffering from this condition, but from erysipelatous inflammation of the scrotum and penis, depending either on defective general health, or on some other cause not connected with the escape of urine into the cellular tissue. He was led to this conclusion by the fact that the

patient had experienced no difficulty in passing his water till there was sufficient swelling of the parts around the urethra to account for it, and by the condition of the perineum, which was quite normal. Besides, he had on more than one occasion met with cases which resembled extravasation of urine as closely as this did, but which proved to be instances of erysipelatous inflammation occurring independently of that accident.

Mr. Paget then proceeded to make such incisions as were necessary to relieve the tension of the parts, and to introduce a catheter into the bladder. A No. 10 passed very readily without meeting with any obstruction; and a few ounces of turbid urine were removed. The after course of the case was that the man fell rapidly into a condition of extreme asthenia, and, although he took a large amount of stimulants, died at 4 A.M. on September 21.

At the after-death examination, the urethra was laid open in its whole length, and found to be perfectly sound. No sign of extravasation of urine could be discovered; and the tissues immediately surrounding the canal seemed less infiltrated than those nearer the surface. The heart was fatty, though only to a small extent; the lungs were healthy; the liver large and fatty; the kidneys rather large, pale, and indistinctly granular.

For the following case I am indebted to the kindness of Mr. Paget, who has allowed me to copy it from his note-book.

CASE II.—*Perineal Abscess, with Stricture—Imitating Effusion of Urine.* Under the care of Mr. PAGET. A man, 45 years old, intemperate, poor, and dull-witted, was admitted into Kenton's Ward in September, 1860. He had had stricture of the urethra for many years, for which many instruments had been passed. His water-strain, he said, had not of late been worse than usual. He had a large and painful swelling, of several days' formation, in the middle of his perineum. His scrotum was swollen to five or six inches in diameter, tense, pallid, glossy; similiar swelling extended from it along both groins, and the tissues felt tense, brawny, and cedematous nearly to the umbilicus. The pubes was also swollen, and in a less degree the integuments of the penis. It was not doubted that this was a case of effusion of urine; it had every feature of a very bad case of the kind. An incision was immediately made into the perineum, and this let out two ounces (about) of very stinking purulent fluid. It came from an irregular cavity in the perineum, which extended thence as far as the finger would reach in the deep subcutaneous tissue of both groins. As it seemed, therefore, that the one incision would relieve both the perineum and the groins, no cuts were made into them, but several small punctures were made into the scrotum. The man had immediate relief from his pain and distress; but two or three days later separate centres of suppuration appeared in the right groin, and though these were opened, extensive sloughing of the integuments and subcutaneous tissue ensued, exposing, after the separation of the sloughs, the abdominal muscles. Similar sloughing afterwards ensued in the scrotum on both sides. With these things he became very feeble. He had daily sickness, frequent diarrhoea, and five weeks after his admission he died, very emaciated and as if exhausted with his local disease and with uræmia. His urine-strain, however, he always said, never troubled him, and no urine passed through the perineum at any time, though the perineal incision remained open.

Mr. Paget here remarks: "Except in this absence of the usual escape of urine by the perineum, the whole course of the disease had

seemed to indicate the consequences of effusion of urine in which there had been unwise neglect of the practice of cutting freely into all the parts into which the urine had penetrated; but on examining, twenty-four hours after death, there appeared no trace or any indication of any communication between the urethra and any part of the suppurating or sloughing cavities in the perineum, scrotum, or groins. These cavities had the appearance of ordinary irregular abscesses, and at their nearest their wall was about a quarter of an inch off the most anterior part of the urethral stricture. In the urethra, just behind the triangular ligament, was an irregularly barred stricture, about two-thirds of an inch long, which would admit a No. 6 catheter; but its walls were entire and firm, without trace of ulceration or any recent inflammatory change, both in front of it and behind it."

The bladder was very thick walled, and its mucous membrane mammillated and blotched with vascularity. Both kidneys were small, lobed, and seamed, tough, pale, very granular, with closely adherent capsules, several small yellow cysts, and wasted cortical substance—marked examples of the last stage of granular degeneration.—*Medical Times and Gazette*.

3. Concussion (?) of the Spine.

In a recent number of *The Lancet* we referred, under the above title, to the case of a man now lying in King's College Hospital. On the 7th instant we again visited him, and, through the kindness of Mr. Bond, the house surgeon, were supplied with further information upon his case. We found him materially improved as regards his power of speech, but unaltered in respect to the paraplegic symptoms already recorded. For a fortnight previous to our visit galvanism, applied in a variety of directions through the body, had been used daily. Two or three days after its first application speech returned. He can now express himself clearly, and with no evident difficulty, except in one respect: his pronunciation of the lingual "th" would be intolerable even in a Frenchman. If we examine this defect it presents a very interesting feature. The tongue, in effecting this sound, has need of greater mobility and power of protrusion than in the production of any other. It is advanced so that the upper incisors are touched, the tip projecting slightly beyond them, and is then retracted suddenly. These movements are necessarily effected through the hypoglossal nerve, the lowest of those which originate in the cephalic prolongation of the spinal cord, generally known as the medulla oblongata, and which approaches in some animals so closely to the regular type of the spinal nerves. We have here, then, impairment of a portion of the spinal nervous system, as shown by the paraplegia, and of just that part of the cerebral system which is, as it were, on the border territory between the two.

Three weeks since, as it had been suggested by more than one observer who had visited this patient that he was shamming, Mr. Bond, with a view to the elucidation of this point, placed him under chloroform. This was a week previous to the return of speech. Whilst insensible he struggled violently with the upper portion of the body, hitting out wildly with his arms, but *never moved his legs*. He made a great noise, but nothing like an articulate sound. He has had no recurrence of the epileptoid seizures described in our previous notice. There is absolutely no sensation in the lower extremities when they

are touched or pinched; but he tells us that he can feel the passage of the electric current quite deeply, and as though in the bones.

We hope shortly to publish some communications which we have received in reply to our request for cases illustrating the subsequent history of patients injured by blows upon the back. Meanwhile we would again draw attention to the importance of such cases being recorded, and invite further contributions. Such reports should include the kind of violence exerted, the immediate symptoms, and the after results, whether of a few months' or years' standing. By an accumulation of such histories, much light may be thrown upon a subject at present involved in obscurity, and great assistance rendered in the fair estimation of the amount of damage inflicted in cases of railway accident.—*The Lancet*.

4. Remarkable Repair of Extensive Injuries.

On the 29th of July last we saw a boy, fourteen years old, admitted into St. Bartholomew's Hospital, whose condition seemed to hold out scarcely a hope of recovery. He had got entangled in some cord-making machinery, and had been rolled by a revolving cylinder much as linen is pressed in a mangle. The ecchymosis was so extensive that his face was enormously swollen and of a dark purple color, the conjunctivæ intensely chemosed, and the features unrecognizable. The aspect resembled that of a very bad case of scurvy, or perhaps still more, as Mr. Paget remarked, that condition of dark tumefaction which sometimes precedes the eruption in malignant variola. The boy had sustained, besides, the following injuries: oblique fracture of the left femur; separation of the epiphysis of the right femur; wound near the left elbow-joint, possibly entering it; dislocation of the right humerus into the axilla. There was concussion of the brain and he lay for many hours in a state of complete collapse. A sixth of a grain of morphia was injected subcutaneously, and this was continued daily. On the 26th of August we found him looking cheerful and completely altered in appearance; there was scarcely any ecchymosis remaining; the wound about the elbow-joint had healed; the left femur was encased in a gypsum splint, and the right had united. When we saw him last, on September 27th, the injuries described were repaired, but he was suffering from suppuration in the neighborhood of the right shoulder-joint. Mr. Eccles, the house surgeon, who had charge of him, tells us that after the reduction of the dislocation, which was easily effected, inflammation took place, and a large abscess formed, which was opened over the pectoral muscle and in the outer wall of the axilla. It is probable that extensive extravasation of blood took place into the joint at the time of the accident. He still complained of stiffness about the back.

The case furnishes a remarkable instance of the elasticity of life at this particular age. A few years later, and the lad's hardened tissues would have been crushed probably beyond repair.—*The Lancet*.

5. Painful Ulcer of the Rectum, with Contraction of the Anal Orifice, and Vascular Polypus.

A few weeks since we saw Mr. Henry Smith operate in King's College Hospital upon a middle-aged female who had been suffering for two years with difficulty in evacuating the contents of the rectum, ac-

accompanied with great pain at the time, which persisted for four or five hours, and then ceased, leaving the patient free until the next action of the bowels. Mr. Smith expected to find simply a fissure of the anus, but, on examination, he discovered that the anal orifice was so contracted as scarcely to admit the point of the forefinger. On inspecting the parts more minutely, a small linear fissure was noticed just within the sphincter, at the posterior verge of the anal orifice. When the patient was placed under the influence of chloroform, the finger could be passed into the bowel, and it was then discovered that a polypus, about as large as a nut, was attached to the posterior wall of the rectum, three-fourths of an inch from the anus. The operation consisted, first, in seizing the polypus with a hooked forceps, bringing it down outside the anus, passing a ligature around it, and then dividing the pedicle. A straight probe-pointed bistoury was next introduced, and the sphincter ani was thoroughly divided in the median line posteriorly, so as to allow of the free introduction of a bougie into the bowel. The patient left the hospital a fortnight after the operation, completely relieved.—*The Lancet*.

6. *Case of Subcutaneous Venous Nævus of the Cheek, with Phlebolites in the Interior.* Under the care of MR. WOOD.

Frances S., aged 17, admitted September 5, 1865. A rather delicate looking girl, with a large swelling on the right cheek, reaching from the orbit to the lower jaw, and from the corner of the mouth to the ear. The tumor is soft, yielding, and elastic, and can be compressed and diminished in size like a sponge. At the upper and back part is a bluish area, of the size of a penny, in which the outline of tortuous veins can be distinguished. A little below the middle, two hard oval movable bodies (phlebolites) can be felt in the interior of the tumor. These can be pushed a little from their places, but afterward assume their original position. The action of the muscles on that side is impeded to some extent, and the right corner of the mouth hangs below the other. States that she has had a swelling there from infancy, but that it has increased somewhat rapidly of late.

On Saturday, September 9th, under chloroform, Mr. Wood applied his subcutaneous clove hitch to the tumor. The object of this ligature is to strangulate the varicose parts with as little damage to the skin as possible. The thread was introduced through the skin at the upper and back part of the circumference of the tumor, and brought out in the inside of the cheek, at the lower and front part, in such a way as to avoid the duct of the parotid opening opposite the second upper molar. A stout semicircular needle, mounted on a handle, (Wood's,) was first passed around the upper half of the circumference of the tumor, close under the skin, and brought out on the inside of the cheek. A stout hempen ligature, well waxed and soaped, was then attached, and drawn back with the needle, leaving one end in the mouth. The needle carrying the other was then passed deeply across the base of the tumor diametrically, carrying a loop of thread; this was seized and retained as the needle was withdrawn. The needle was then passed close under the skin round the remaining part of the circumference, the end of the thread seized and detached, and the needle withdrawn. In each case the needle entered at the same aperture in the skin, and emerged by a single opening in the mouth. Through this latter, both the ends and the loop of the thread were at

this stage of the operation hanging out. The ends of the thread were then passed separately, and in opposite directions, through the loop, and then tightened up, and tied in a bunch. The tightening produced a marked impression upon the bulk of the tumor, and depressed the skin all around.

Two days after there was a considerable amount of swelling of the face and eyelids, which were œdematous. Complained of much pain, which was relieved by Battley's fluid. Suppuration then set in profusely, emerging freely at both openings, but chiefly through the mouth. Shreds of lymph were occasionally discharged. About ten days after, a small abscess made its appearance at the border of the lower jaw from gravitation of pus. This was opened by a small puncture, and well squeezed every day. It was hoped that the phlebolites would become squeezed out into the mouth in the course of the daily manipulations, but this did not occur. When the ligature came away, which it did in a small loop about fourteen days after the operation, the hard bodies could be felt adherent, and not movable, close under the mucous membrane of the cheek, whence they may easily be removed by a slight incision, if necessary. The whole mass has now become solidified and hardened throughout; a slight discharge passes out at each of the punctures. The movement of the muscles of the cheek and mouth is considerably impaired from the division of the plexuses of the portio dura. This and the induration will doubtless be in time entirely overcome.—*Medical Times and Gazette*.

7. *Complete Crushing of all the Soft Parts, and of both Bones of the Leg, from the Passage over the Limb of a Heavily Laden Wagon—Primary Amputation above the Knee—Recovery. Under the care of Mr. WORMALD.*

E. N., aged 48, a very muscular, rather free-living man, was admitted into the hospital on August 22. He had been knocked down by a very heavy wagon, the wheels of which had passed over his right leg. The limb was completely crushed; the tibia and fibula could be felt broken in several places; the calf was much swollen and discolored from effused blood, and a large extravasation of blood had occurred round the knee. Mr. Wormald at once amputated through the middle third of the thigh. The method of amputation was by skin flaps, and a circular division of the muscles. The wound was not closed at first, but sutures were inserted through the flaps, and left loose, to be tightened at the end of a few hours, when the surfaces should have become glazed, and if there appeared to be no threatening of hemorrhage. No hemorrhage occurred, and the sutures were tightened on the following day, (August 23.)

The note taken on August 24 says: "The patient has been doing remarkably well. The expression of his face is almost that of ordinary robust health; pulse 84, quiet, rather full; skin only moderately hot from reaction; tongue scarcely at all coated. He had a good night."

After this date the patient had no check of any kind; at no time after the amputation was his pulse above 96; generally it was below 80. He took an ordinary meat diet with relish, slept well throughout the night, and always said he was quite well. The stump healed quickly. He left the hospital, at his own request, on October 9th.—*Ibid*.

8. *Compound Comminuted Fracture of both Bones of the Leg, with Extensive Laceration of the Soft Parts—Continued Hemorrhage—Amputation—Recovery.* Under the care of MR. COOTE.

J. L., a big, heavy brewers' drayman, drinking, as his customary allowance, two gallons of beer a-day, had his leg crushed between the wheel of his dray and an iron post at the corner of a street, on September 1. A compound comminuted fracture of both bones, with great bruising of the muscles of the calf, was the result. He was brought about four hours afterwards to the hospital. Hemorrhage, which, it was said, had been to a large amount, was still going on. The limb was placed on an iron back splint, and the wound (about two inches long) closed with lint soaked in blood. As both the main arteries of the leg could be felt pulsating, it was hoped the bleeding might cease; it still continued, however, and the calf was soon much discolored and swollen by the infiltration of blood beneath the skin and among its lacerated muscles. As there was no hope of saving the limb under such circumstances, Mr. Coote proceeded, twelve hours after the accident, to amputate just below the knee. The operation was performed by making a short anterior and a long posterior flap and a circular division of the muscles. The flaps were brought together by a single central silver suture, and bands of strapping. It was found, on examining the leg after its removal, that the tibia and fibula were fractured in several places, and the muscles lacerated in their whole extent. The anterior and posterior tibial vessels were entire; the exact source of the hemorrhage which had occurred could not be discovered.

For the first thirty-six hours after the operation the patient remained quiet and very drowsy; reaction then commenced, and was well established on September 3, when he was hot, flushed, and still drowsy, with a red tongue, rather brown in the centre, and a full, bounding pulse of 120. The stump looked rather sodden and cedematous, and a large amount of sanious fluid was being discharged. No symptom of delirium tremens, to which his previous habits had made him liable, appeared. He took his diet well; this consisted of two pints of porter, four ounces of brandy, beef-tea, milk, and two eggs.

By September 5th all excessive reaction was passing off. The pulse was quiet, 108; the skin cooler; and a healthy suppuration was beginning in the stump; the patient slept well at night, and expressed himself as feeling free from uneasiness. From this date he went on uninterruptedly to recovery, and was able to leave his bed on Oct. 2d.

A few days ago, when the amputation wound had all but healed, he fell in the ward and injured the stump; this accident, however, promises to detain him in the hospital only a few days.—*Ibid.*

9. *Sympathetic Ophthalmia.*

In an essay on Sympathetic Ophthalmia, Mr. Lawson gives the following as his general conclusions: 1. It is an inflammation of one eye, originating solely from an irritation in the other. 2. The most frequent cause of sympathetic ophthalmia is a wound of one eye, and that those wounds which involve the ciliary region are especially liable to produce it. It also frequently arises from the irritation communicated to the sound eye from a lost eye, or from the shrunken stump of a lost eye, which has become subject from some cause or other to recurrent attacks of inflammation. 3. One of the great

peculiarities of sympathetic inflammation is its tendency to the rapid effusion of lymph into all the tissues of the eye which it renders incapable of speedy organization. 4. The disease once started is very difficult to arrest; it is recurrent in its nature, and even if the first attack be arrested, a recurrence is almost certain to take place. 5. The removal of the injured eye affords the best chance of arresting the disease; and if this operation is resorted to in its very early stage, there is a good prospect of its doing so. 6. If the symptoms of sympathetic ophthalmia are fully developed, the removal of the injured eye (the source of the irritation) may fail to arrest the disease, though it will afford a chance which should not be neglected. 7. In no instance have I seen sympathetic ophthalmia *originate* in an eye after the other had been removed on account of an injury. I have frequently seen it continue its course after the removal of the injured eye, but in each case sympathetic symptoms were manifested before the eye was removed. Hence the importance of diagnosing in what cases of injury sympathetic ophthalmia is likely to follow, and the necessity of at once excising such injured eyes which are prone to produce it, and especially if they are already lost for all visual purposes. 8. In the treatment of sympathetic ophthalmia, any operation whilst the eye is inflamed is positively prejudicial; but when all the activity of the disease has subsided, much may often be done by some operative procedure to regain for the patient some of the sight he has lost, and often also at the same time to prevent a recurrence of the attacks. 9. In the early stage of the disease the tension of the globe is often increased to + T 1 or 2; but in the latter stage the eye becomes soft from atrophy of the vitreous body, causing a diminution in its consistence and bulk, and this state is often followed by detachment of the retina.—*British Medical Journal*.

10. *Umbilical Hernia; Sloughing of Four Inches of the Small Intestines; Complete Recovery.*

The following case, which occurred in the practice of Dr. Nolan of Wicklow, was read by Dr. Benson, at a meeting of the Surgical Society of Ireland. James Delany, a man about 50, was admitted into Wicklow Infirmary on June 16th. He had an umbilical hernia of about a twelvemonth's standing. Eight days before admission, in struggling to hold a pig, he felt something give way at the tumor, was seized with weakness, followed by pain, and soon after had vomiting. In this state he continued for seven days, using such means as his friends suggested, till the seventh day, when the medical officer of the district was called to visit him. Seeing the state of the case, and the man being in a remote part of the district, he recommended him to be conveyed to the infirmary, to which he was brought on a car, a distance of about seven miles. Dr. Nolan saw him on the eighth day, and found a hernia at the umbilicus about the size of a largish orange, a black mass, with a line of separation forming at the base, and a blush of redness in the surrounding integuments, especially towards the left side; the pulse was weak, the countenance pale and anxious, the stomach gulping up every thing. Manual interference was out of the question. He therefore determined to leave to the adhesive process the repair of the local damage, and to allay irritation and support the patient's strength, ordering a grain of opium every fourth hour till the vomiting ceased; beef-tea, and brandy and water in small quanti-

ties, and a linseed-meal poultice over the tumor. Next day he had slept well, and was free from pain; the vomiting had ceased after the second pill. The opium was discontinued. The beef-tea, etc., had remained on the stomach; the countenance and pulse were improved; the whole of the integuments had sloughed away, disclosing between three and four inches of small intestine completely disorganized and ready to slough, which it did in two days after, followed by a discharge of bilious curdy fluid. The treatment from this time consisted of giving as much beef-tea and brandy and water as he could take, and throwing up an enema daily of strained gruel and milk, which was generally retained till next day. In about a week the opening began gradually to contract; in a fortnight it had closed; the man daily improved in health and strength, the bowels acted naturally when the enemata were discontinued, and he was enabled to leave the hospital on July 22d, a month and five days from his admission.—*Dub. Med. Press.*

EDITORIAL.

—The cholera seems, by report from all sources, to be on the decrease in Europe, although many new places are mentioned as having been recently infected. No satisfactory accounts are given of the epidemic in France, the government keeping a most reserved silence upon the subject. We learn, however, from late foreign medical journals, that there are no more new cases in Toulon and Marseilles, while in Paris there is a daily diminution of cases.

The *British Medical Journal* of the 11th November, quoting from the *Union Médicale*, says:

“The cholera decidedly seems to be disappearing; not suddenly, and to return in the same manner, as was seen in preceding epidemics, but gradually. All alarm should therefore cease, but without salutary precautions being forgotten. On the 1st instant, the total number of deaths from cholera in private houses and in the hospitals was only 92; on the 2d, 80; on the 3d, 75; and on the 4th, 70. The civil and military hospitals are down for only about a quarter in that total. The number of admissions to the hospitals has diminished in proportion to that decrease, and the number of home cases is lessening. The epidemic appears, therefore, to be fairly dying out. If the cholera thus stays its ravages, it will have been relatively mild as compared with preceding invasions; and this satisfactory result must be attributed to the improvements in the capital, and the disappearance of the numerous and unhealthy quarters of Paris in which the epidemics of 1832 and 1849 made so many victims.”

In England, the Registrar-General reported three deaths from cholera for the week ending October 28. No more deaths up to November 1.

An important movement has taken place, instigated by the French government, for the investigation of the epidemic at its apparent source in the East, and other governments of Europe have been urged to join in the movement. Delegates from the respective European governments, together with "such men of science thought the best fitted to assist in deliberations by their especial knowledge," will accordingly meet in Constantinople for this purpose, and it is to be hoped that some system may be devised by which this great evil may be banished from the abodes of men.

Notwithstanding the apparent reticence of the French government in relation to the rise and progress of the epidemic within its own borders, the Public Council of Hygiene and Salubrity for the Department of the Seine has issued a series of instructions in relation to the precautions to be adopted against attacks of the epidemic.

The instructions, signed Jobert de Lamballe, Vice-President, Boitelle, Préfect of Police, recommend: 1. Tranquillity of mind; 2. Moderate, healthy and regular nourishment of due substantiality; 3. Clothing which will protect against sudden transitions of temperature; 4. Salubrity of dwelling houses: free ventilation, moderate temperature, avoidance of overcrowding; 5. Avoidance of bodily fatigue, lengthened studies, late hours, and abuse of pleasure; 6. Attention to the earliest symptoms of diarrhœa.

"It may be affirmed, with rare exceptions," continue the instructions, "however sudden the attack may be, cholera is yet preceded by symptoms which may induce fears as to its development. The commonest of these symptoms is diarrhœa, however slight, and such is its importance that the removal of this symptom immediately on its development will suffice to prevent the malady. It would, therefore, be dangerous to let the diarrhœa continue, and consequently, as it may be arrested by very simple means, they may be resorted to before the arrival of the doctor, who ought always to be sent for without delay. These means are as follow: Partial or complete abstinence from food, the use of rice and its preparations, an infusion of chamomile tea, and copious injections with a decoction of marshmallow and unprepared starch. The majority of the cases observed hitherto show that the chances of recovery are lessened or increased according to the length of time that may elapse between the first manifestations of the disease and the administration of relief. It is, therefore, necessary to specify the chief symptoms which announce the attack, and to indicate the mode of relief to be followed immediately on the appearance of the epidemic. Cholera is usually announced by a profound and sudden feeling of lassitude, colics, diarrhœa, with stools first colored and then colorless, resembling rice water, nausea and vomiting, a very marked change in the features of the face, coldness in the body and the tongue, cramps, and, lastly, a bluish appearance in the lips and

face. The instant any of these symptoms show themselves a physician must be called, and, while awaiting his arrival, the means adopted must be the following: The skin should be heated, and warmth obtained by placing a bottle of hot water or heated bricks wrapped in sheets at the feet of the patient and between the legs. He must be covered up in warm sheets and several blankets, between which hot irons or a warming-pan should be agitated up and down, so as to act upon the whole surface of the body. While these steps are in preparation, or even in progress, the limbs should be rubbed for a considerable time with force with the palm of the hand, a soft brush, or a piece of flannel—which latter may be moistened with camphorated brandy, brandy alone, or Eau de Cologne—and this should be done by two persons seated at each side of the patient, taking care not to uncover him. The drink given should be a hot infusion of linden tea, or peppermint tinctured with a few drops of brandy. Should these tisanes, however, appear to increase the vomitings, gaseous water or ice in small pieces might be advantageously resorted to, and sinapisms drawn over the legs and thighs. It will always be well, if possible, to let the patient lie in a separate room, so as to place him in the most favorable conditions for salubrity. The precautions to be taken during convalescence will be intimated by the physician who may be called in. The urgency, however, can not be too strongly impressed upon convalescents of rigorously observing the rules for preservation given above. They must be specially careful to avoid cold, damp, and change of regimen, for persons who have been attacked by cholera are exposed to relapses."

In relation to the premonitory diarrhœa, Dr. Chauffard, of the Children's Hospital at Paris, says: "That the present epidemic of cholera was not preceded by diarrhœa in Paris, and that the same is true of the cholera in the South of France. The diarrhœal affections appeared immediately subsequent to the cholera. The statement, therefore, that diarrhœa always precedes the advent of cholera, can not be taken as true"

Following the avenues of commerce this disease has, at last, reached our own shores. On the 3d of November the Atlanta arrived at this port from London *via* Havre, bringing a goodly complement of cabin and steerage passengers, the latter mostly immigrants, from Germany, many of whom had passed through a portion of France, and had remained for a longer or shorter period of time in Paris. During the passage across the Atlantic the disease declared itself, several deaths occurred, and by the time the vessel reached this harbor it had assumed a severe type. It was limited, however, to the steerage, and up to the present time, by proper sanitary precautions, the cabin passengers have been spared from any sickness from this cause. The vessel and its passengers were immediately put into the closest quaran-

tine, and every precaution adopted to prevent the propagation of the disease, both on board the vessel and beyond its decks—thus far with most entire success in limiting it to the infected vessel.

— **TRACHESTASIS IN A DUCK—CORRECTION.**—The following extract from a communication to one of the editors, by Prof. Joseph Leidy, of the University of Pennsylvania, explains itself. Prof. Jeffries Wyman, of Harvard University, has also noticed the same error.

“In perusing the June number of the **NEW YORK MEDICAL JOURNAL**, my eye was caught by the heading, ‘Trachestasis in a Duck,’ (vol. I., p. 221.) I was rather amused to find that the author had described the normal condition of the inferior larynx and other parts of the male duck as one of disease. It reminded me of a somewhat similar circumstance which occurred several years since. A medical student exhibited to several of the professors of the University the inferior larynx of a duck, as an ossified heart of the animal.”

— **Medical Journalism** is being revived in the Southern States. Two new journals are announced, one in Richmond, Va., and the other in Savannah, Ga. The Virginia journal, which is to be called the Richmond Medical Journal, will commence with the new year, the first number appearing early in December, and will be edited by Drs. E. S. Gaillard and W. S. McChesney. The journal will be a monthly of 80 or 90 pages, octavo form.

The Georgia journal will be a revival of the Savannah Journal of Medicine, under the auspices of the Georgia Medical Society, and will be edited by Drs. Juriah Harris, Jas. B. Read, and J. G. Thomas. It is to be published bi-monthly, and will consist of 72 pages, octavo form.

We cordially welcome these new journals, and wish them every meed of success.

— The Lectureships in the Medical Department of the University of Pennsylvania, recently endowed by Dr. Geo. B. Wood, of that city, have been filled by the appointment of the following gentlemen:

Dr. J. J. Reese to that of Medical Jurisprudence, including Toxicology; Dr. Henry Hartshorne to that of Hygiene; Dr. J. C. Hayden to that of Mineralogy and Geology; Dr. H. C. Wood to that of Botany; and Dr. Harrison Allen to that of Zoology and Comparative Anatomy.

— Prof. Malgaigne, who was seized with some cerebral disorder while occupying the chair as President of the Paris Academy of Medicine, a year since, recently died from a third attack, not yet having reached his sixtieth year.

BOOKS AND JOURNALS RECEIVED.

Lectures on Inflammation: being the first course delivered before the College of Physicians of Philadelphia, under the bequest of Dr. Mütter. By John H. Packard, M.D., &c. Philadelphia: J. B. Lippincott & Co. 1865.

Materia Medica, for the use of Students. By John B. Biddle, M.D., Prof. of Materia Medica and General Therapeutics in the Jefferson Medical College, &c. With Illustrations. Philadelphia: Lindsay & Blakiston. 1865.

The Practice of Medicine. By Thomas Hawkes Tanner, M.D., F.L.S., &c. From the Fifth London Edition. Enlarged and Improved. Philadelphia: Lindsay & Blakiston. 1866.

Stimulants and Narcotics; their Mutual Relations with Special Researches on the Action of Alcohol, Ether, and Chloroform on the Vital Organism. By Francis E. Anstie, M.D., M.R.C.E., &c. Philadelphia: Lindsay & Blakiston. 1865.

On Wakefulness, with an Introductory Chapter on the Physiology of Sleep. By William A. Hammond, M.D., &c. Philadelphia: J. B. Lippincott & Co. 1866.

Chloroform: its Action and Administration. By Arthur Ernest Sansom, M. B., Lond., &c. Philadelphia: Lindsay & Blakiston. 1866.

Lectures on Epilepsy, Pain, Paralysis and Certain other Disorders of the Nervous System. By Charles Bland Radcliffe, M.D., &c. Philadelphia: Lindsay & Blakiston. 1866.

Epidemic Cholera, Diarrhoea and Dysentery. Pointing out an Effectual and Expeditious Method of Cure. With cases submitted to the Medical Council of the Board of Health, during the Epidemic of 1854. By Henry Jeanneret, M.D., &c. London: George Philip & Son. 1857.

De La Guérison Prompte et Facile du Cholera Asiatique par la Méthode de Henry Jeanneret, M.D., &c. Cheltenham. 1865.

Experimental Investigations into the Action and Uses of the Bromide of Potassium. By Robert Bartholow, M.D., &c. Reprint.

Reports of the Trustees and Superintendent of the Tennessee Hospital for the Insane; presented to the General Assembly, April 3, 1865. Nashville. 1865.

A Report upon the Epidemic occurring at Maplewood Young Ladies' Institute, Pittsfield, Mass., in July and Aug., 1864, including a discussion of the causes of Typhoid Fever. By A. B. Palmer, M.D., C. L. Ford, M.D., and Pliny Earle, M.D.

Suggestions relative to the Pathology of Pneumonia. By S. O. Vanderpool, M.D. Reprint.

A Catalogue of Medical Works on Cholera, in the Library of J. M. Toner, M.D. Washington, D. C. 1865.

Edinburgh Medical Journal. July, Aug., Sept., Oct.

Glasgow Medical Journal. Oct., 1865.

British Medical Journal. Sept. 23, 30; Oct. 7, 14, 21, 28; Nov. 4, 11.

The Lancet. Sept. 23, 30; Oct. 7, 14, 21, 28; Nov. 4, 11.

The Medical Circular. Sept. 6, 13; Oct. 4, 19; Nov. 1, 8.

Medical Times and Gazette. Sept. 23, 30; Oct. 7, 14, 21, 28; Nov. 4, 11.

The Medical Press, Dublin. Oct. 4; Nov. 1, 8.

American Journal of Insanity. Oct.

American Journal of the Medical Sciences. Oct., 1865.

American Druggists' Circular.

Boston Medical and Surgical Journal. June 29; July 20, 27; Aug. 17, 24; Sept. 7; Oct. 5, 12, 26; Nov. 2, 9.

Buffalo Medical and Surgical Journal. Oct.

Chicago Medical Examiner. Oct., Nov.

Chicago Medical Journal. Oct.

Cincinnati Lancet and Observer. Oct.

Pacific Medical and Surgical Journal. Oct.

The Medical News and Library. Oct.

American Literary Gazette and Publishers' Circular. Oct. 2, 16; Nov. 1.

Medical and Surgical Reporter, Vol. XIII. Nos. 1-21.

Anales de la Real Academia de Ciencias Medicas, Fisicas y Naturales de la Habana: Agost, Sept., Oct.

NEW YORK MEDICAL JOURNAL,

A MONTHLY RECORD OF MEDICINE AND THE COLLATERAL SCIENCES.

JANUARY, 1866.

ORIGINAL COMMUNICATIONS.

Remarks on the Temperature of some of the Invertebrates. By
J. S. LOMBARD, M.D., Boston.

The temperature of the invertebrates has been more or less carefully studied in Europe by Dutrochet,* Becquerel,† Valentin,‡ Newport,§ J. Davy,|| and others. In the United States, however, I believe that no particular attention has been given to this subject; and although several of the foreign observers have made use of thermo-electric apparatus in their researches, yet, in this country, so far as I know, the investigations given in the present article are the first in which thermo-electricity has been systematically applied to the determination of the temperature of the animals in question.

In publishing these investigations, I am fully aware of their incompleteness, and my reason for having them printed is not that I consider them of so much value in themselves, but that I may in this way call the attention of those interested in phys-

* Ann. d'Hist. Nat., 2d series, Zoologie, t. xiii.

† Traité de physique.

‡ Répert. de Anat. et de Physiol., 1839, t. iv.

§ Philosoph. Transactions, 1837.

|| Researches, Physiological and Anatomical. Vol. 1st.

iological science to a subject that has been very much, if not wholly, neglected here, and especially to the application of thermo-electricity to the study of the phenomena of animal heat in general.

In determining the temperature of the invertebrates, we may use either the thermometer or one of the various forms of thermo-electric apparatus. The latter are, however, always to be preferred, being far more delicate, and likewise more convenient than the former. But if good thermo-electric apparatus and a sensitive galvanometer can not be obtained, we may, if the animal be sufficiently large, take its temperature by introducing the bulb of a very delicate thermometer into its interior, either through the natural openings or through an incision made for the purpose, the latter of the two methods being the one usually adopted by J. Davy.* If the animal be too small to admit of the introduction of the thermometer in either of the above ways, Mr. Newport's plan† may be pursued, which was to apply the external surface of the animal (in his experiments, an insect) to the bulb of the thermometer by means of a pair of pincers. These latter should be of sufficient length to prevent the radiation of heat from the hands from affecting either the animal or the thermometer, and the handles, if metallic, should be wrapped with some non-conducting material, in order to guard against the possible transmission of warmth by conduction. To avoid the effects of radiation, not merely from the person of the observer, but also from surrounding objects generally, Mr. Newport enveloped both insect and bulb with wool.

There is, however, an objection to this last procedure. The air entangled in the wool, not being changed with sufficient rapidity, becomes heated by the insect, and in turn reacts upon the latter, and the result is, that the animal exhibits a degree of heat which it could not, under ordinary circumstances, maintain when freely exposed to the atmosphere. This objection also applies to those cases in which the animal is examined in a glass tube or other vessel of small capacity.

There is also another objection to Newport's method. All investigations made by placing the external surface of the

* Op. cit., p. 195. † Op. cit.

animal in contact with the thermometer are faulty, for the very obvious reason that we do not in this way obtain the temperature of those portions of the organism in which the production of heat is greatest, the external surfaces of all animals being, as a rule, considerably cooler than their interiors.

Experiments conducted in the manner of Mr. Newport can, therefore, simply prove the existence of a heat-producing power, but can not give us the proper temperature of an animal.

If, instead of the thermometer, we use thermo-electric apparatus, we have our choice of several different instruments.

Nobili and Melloni* employed the thermo-electric pile, and by means of it first detected the presence of heat in insects. For this purpose they fitted to the faces of the pile small chambers of brass, in one of which the animal was placed, in the focus of a little reflector. The heat radiated from the animal fell upon the face of the pile, causing a deflection of the needle of the galvanometer, proportional, of course, to the difference in temperature of the two faces.† This method is manifestly objectionable on the same grounds as that of Newport, inasmuch as the animal is confined in a small volume of air, and the instrument does not obtain the temperature of the interior organs.

Still the pile is a very useful instrument for simply proving the existence of an independent source of heat in the inferior animals, when all other means have failed. Although the instruments to be presently described possess the advantage that they can be introduced into the interior of the organism, yet the pile is so much more delicate that it is capable of indicating differences of temperature which the other instruments, in spite of their contact with warmer organs, are unable to detect.

Instead of following the plan of Nobili and Melloni, I have generally secured the animal (always an insect) directly to the face of the pile, by ligatures, or by strips of adhesive plaster. In doing this great care must be taken to prevent the ligatures

* Treatise on Electricity by A. De La Rive. Vol. ii., part v., p. 571. Also, *De la Chaleur produite par les etres vivants*, par J. Gavarett, p. 117.

† This is not strictly true. The deflection of the needle is exactly proportional to the force of the current only up to a certain number of degrees, which has to be determined for each instrument.

or plaster from touching the antimony and bismuth bars, and also to prevent any of the fluids of the insect from coming in contact with these bars; in the case of grasshoppers this latter precaution is especially necessary.

Becquerel and Dutochet invented instruments much better adapted to the determination of animal temperature than the pile, excepting in extraordinary cases.

Becquerel* employed two needles precisely similar, each about the twentieth of an inch in diameter, and composed half of steel and half of copper, one extremity being steel and the other copper. The centre of this compound needle consisted, therefore, of a soldered junction of two dissimilar metals, and consequently formed a thermo-electric battery.

One of these needles was pushed through the body of the animal under examination, until the junction reached the desired depth; its steel point was then connected with that of the other needle by means of a steel or iron wire—this second needle being left exposed to the air, merely taking the precaution to protect its junction from disturbing influences by means of a roll of dry paper. Lastly, the two copper extremities were connected with the galvanometer, and the needle of this latter instrument indicated by its deflection the difference in temperature of the two junctions.

These needles of Becquerel, although often serviceable, are yet, in many respects, inconvenient.

In the first place, we are always obliged to push the steel point entirely through the part the temperature of which is to be taken, in order that it may be connected with the other needle, and this is frequently a troublesome thing to do.

There are other inconveniences attendant upon their use, that can not well be appreciated, except by actual experience.

The apparatus of Dutochet†, like that of Becquerel, had its source of electricity in a copper and steel junction, but this junction formed a sharp point, which could be inserted into the body of the animal. This apparatus is, in many respects, preferable to that of Becquerel.

* Op. cit., t. iv., p. 51, and Treatise on Electricity by A. De La Rive. Vol. ii., part v., p. 579.

† Op. cit., t. xiii., p. 5.

After a long and careful trial of the instruments just described, I devised a substitute for both.

In the first place, I have substituted German silver and brass for copper and steel. The iron or steel of the other instruments is apt to rust, particularly when exposed to an atmosphere saturated with aqueous vapor, as it often is in experiments upon the invertebrates. The metals are in the form of thin plates, three-sixteenths of an inch in width, and two and a half inches in length, to the place where they meet. They are separated from each other by a piece of bone rubber, an eighth of an inch in width at the upper extremities of the plates, but gradually narrowing down to the junction, at which point the two metals are welded together, so as to form either a sharp or blunt end, according to the particular purpose for which the instrument is intended. The insulating rubber runs up for the distance of an inch and a half or two inches above the commencement of the plates, forming a rounded handle, by which the instrument can be conveniently grasped.

The two brass plates are connected (for of course there is, as in Becquerel's needles, a pair of these instruments) by a wire, and the two silver plates connected each with a pole-cup of the galvanometer. Or, if we choose, we can connect the silver plates with each other, and the brass ones with the galvanometer, merely bearing in mind that the current is always from the silver to the brass through the junction, and consequently from the brass to the silver through the coil of the galvanometer.

This apparatus is not only more convenient than those of Becquerel and Dutrochet, but also much more delicate.

Whatever kind of thermo-electric apparatus we use, it is necessary to exercise the greatest care, for, as a rule, the evolution of heat in the invertebrate is feeble, and it is only by sedulously avoiding all sources of error that reliable results can be obtained.

No one should attempt to use thermo-electric apparatus without a thorough knowledge of the principal facts of thermo-electricity and of electro-magnetism. If this knowledge be not possessed, the indications of the galvanometer will be full as often interpreted wrongly as rightly. But even after a

theoretical acquaintance with the subject, it requires considerable practical experience to use the apparatus successfully in the more delicate experiments.

We must always bear in mind that we have not merely two sources of electricity in our circuit, viz., the two junctions, but that *every* deviation from homogeneity constitutes a battery. Every twist in our wires, every soldering, and finally the very brass pole-cups of the galvanometer, all may give rise to electric currents. In some of the very delicate investigations, the junction of the copper wires with the pole-cups of the galvanometer will, unless we be upon the watch, mislead us. It almost always happens, when a very sensitive galvanometer is used, that, at the moment the circuit is closed by connecting the copper wires with the galvanometer, the needle of the latter is deflected to a slight degree by the warmth of the hands communicated to the junction of the copper and brass, the current passing, in this case, from the brass to the copper through the junction.

Many pages could be written on the modes of applying thermo-electricity to the study of animal temperature, and on the errors to be guarded against; but the limits of this article forbid my devoting any more space to this part of the subject.

Having thus glanced at the different methods and means of investigation, let us next consider the particular experiments performed by myself.

Of the three great classes into which the invertebrates are divided, representatives of two, viz., the mollusca and the articulata, have been examined by me. Of the temperature of the radiata, I have no experimental knowledge; indeed, about all that is known of the temperature of these animals is derived from the experiments of Valentin solely.

Commencing with the mollusks, the temperature of the clam has been carefully studied by me. The mode of procedure was as follows:

My thermo-electric instruments (those with blunt junctions were used) were secured each by a brass claw, lined with cork, and fitted to an upright metallic rod in such a manner as to admit of both vertical and horizontal movement.

The instruments were held in the claws by their handles, in

a vertical position, with the junctions down. The animal was first examined in the air. For this purpose it was wiped as dry as possible, and then let alone for half an hour or more. It was then seized by means of a very long pair of forceps, and held directly under one of the the thermo-electric junctions. It was next forced to open its shell, when the claw holding the thermo-electric instrument was moved quickly down the upright rod, until the junction was fairly in the interior of the animal. As the clam invariably closed its shell tightly upon the instrument the moment it was introduced, the latter was held firmly in position. The other extremity of the animal rested upon a plate of glass. The second thermo-electric instrument had its junction protected by a roll of paper or by a piece of cork.

After waiting for a short time, in order to allow all effects produced by the hands to pass off, the connections were made with the galvanometer.

Operating in this way, I found that the temperature of the clam was invariably lower than that of the air, by a half or a quarter of a degree of F.

This was not surprising, considering, on the one hand, the naturally feeble production of heat by the animal, and on the other hand, the very copious evaporation that would necessarily take place from so moist a body, even after having been freed as much as possible from moisture.

In the next place, the temperature of the clam compared with that of the water was obtained, the animal being in the air.

To do this, the animal was quickly removed from the water, and the thermo-electric apparatus applied as in the former case, with the exception that the second junction was lowered into the water.

The result was, that the temperature of the clam was, in every instance, found to be lower than that of the water by half a degree of F., or even more; the influence of evaporation being, in this case, even greater than in the preceding, inasmuch as the clam was dripping wet. Finally, the temperature of the clam, compared with the water, while the animal was actually immersed in the latter, was taken.

Now, it is proper to remark, that although by taking the

temperature of an animal in water we put a stop to evaporation, yet if the volume of water be at all considerable, its high specific heat and better power of conduction render both its cooling and heating properties very much superior to those of dry air of the same temperature. Thus the specific heat of dry air is, according to Regnault, for equal weights, only 0.2375 compared with water as 1.000.*

This accounts for the fact, that Valentin found the temperature of marine animals to be sometimes lower in the water than in the air.†

Of course the greater the mass of water the greater will be its power of refrigeration, and consequently we can reduce this power to its minimum by making use of very small volumes of the liquid.

A glass vessel, but little larger than the clam itself, was therefore taken, and having been filled with water of the temperature of the air, the clam was immersed in it, and left untouched for several hours; at the end of this time the animal was seized with the forceps, and lifted only so far above the surface of the water as sufficed for the proper introduction of the thermo-electric instrument. This done, the animal was again submerged, with the junction still in its body, and at the same time the second junction was lowered into the water.

When all due precautions were adopted, it was generally found that the clam was warmer than the water, this excess never amounting, however, to more than a quarter of a degree of F., and being usually less than this.

During these experiments, as well as during those previously related, the temperature of the air and of the water ranged between 75 and 95 degrees of F.

It may appear, at first sight, that this mode of operating is open to the first of the two objections brought against Mr. Newport's method. It may seem that the clam is examined under circumstances more favorable to the maintenance of warmth than those under which it ordinarily exists, and that therefore, we can not by this method obtain the proper temperature of the animal. Such is not the case, however, for in its

* Miller's Chemistry—Chem. Physics, p. 237.

† Op. cit., p. 259.

natural state the clam lies imbedded in mud, and is, if any thing, protected in greater degree from loss of heat than in the experiment we have been considering.

There is another point of interest to be considered in this connection, and that is, that it is not necessary to find in a living animal a temperature superior to that of the air (provided the latter be not saturated with aqueous vapor) in order to prove the existence of an independent source of heat; for all animals are moist bodies, from whose surfaces a constant evaporation is taking place, and this evaporation tends to keep their temperature below that of the air. A moist, inorganic mass would, under like circumstances, cool down a little below the air, and therefore when we find an animal maintaining, in spite of the depressing influence of evaporation, a temperature equal to that of the atmosphere, we have proof of the existence of a heat-producing power, this proof being more or less conclusive according to the degree of saturation of the air.

I have examined the temperature of several other mollusks, but as the results have not been very satisfactory, I have not considered them worthy of publication, until the experiments from which they were derived shall have been repeated with more care and on a larger scale.

Passing next to the second great division of the invertebrates, viz., the articulates, we will consider in turn each of its subdivisions. First, the annelida.

Of this subdivision, the earthworm and leech have been examined by several observers, and my own experiments have also been made upon the same animals.

Hunter* introduced the bulb of a thermometer into a knot of earthworms, and found them to be a degree and a half or two degrees F. warmer than the air. He also found that the ordinary medicinal leech preserved a temperature one or two degrees above that of the air. Dr. J. Davy† took the temperature of two kinds of leeches in Ceylon, and found it to be the same as that of air. Dutrochet‡ and Berthold obtained results similar to those of Davy.

These few cases are about all that are recorded of the temperature of the annelida.

* Carpenter's Comp. Phys., p. 453.

† Op. cit., p. 195.

‡ Op. cit.

In the experiments performed on the earthworm and on the leech, the same apparatus was employed, and the same general plan pursued as in the case of the mollusks, with the exception, however, that, as a rule, a saturated atmosphere was resorted to, instead of water, as a means of suspending evaporation.

The temperature of these animals varied very considerably. In many instances it was half a degree of F. or more above that of the atmosphere. In other instances the temperature was the same as that of the atmosphere, and again in some cases it was cooler than the air by a quarter of a degree of F. or more. In all these latter cases, the suspension of evaporation was invariably followed by a rise of temperature, which generally continued until the animal was warmer than the air by a quarter or half a degree of F. As a general thing, the leeches were a little superior in temperature to the earthworms.

Concerning the temperature of the second subdivision, the crustacea, but little is known. Valentin has recorded in four specimens of the crab the following temperatures above that of the surrounding medium: 0.54° , 1.08° , 1.62° , 0.18° , F.

Davy found the temperature of a large cray fish to be 79° F. when the air was 80° F., and that of a crab to be 72° F. when the air was at 72° F. likewise.

I have studied the temperature of two members of the crustacea, viz., the lobster and the shrimp.

The temperature of both of these animals is, so far as my own experience goes, invariably below that of the air or water by half a degree of F., or even more. Moreover, although every precaution was adopted, I never succeeded in obtaining positive proof of a heat-producing power in these animals. On stopping evaporation they warmed up somewhat, but never in any case attained a higher temperature than that of the surrounding medium.

There can be no doubt that every living organism, whether animal or vegetable, produces a certain amount of heat. These crustaceans must not, therefore, be considered as wholly destitute of a heat-producing power, but merely as cases in which this power is at its minimum, and consequently extremely difficult of detection.

I have not been able to find any recorded observations on

the temperature of the third subdivision, the arachnida; but from the few experiments that I have made upon spiders, I am inclined to consider their heat-producing powers as quite active; but more investigations are needed to establish this point.

Lastly we come to the insects, which, as a class, have had their temperatures more carefully studied than any other of the invertebrates.

As the results obtained by myself have been in the main confirmatory of those of previous observers, I will merely mention a few points that appear to be of especial interest.

The same instruments were used in these experiments as in those previously related, and in every case both insect and thermo-electric apparatus were covered with a bell jar.

I am inclined to think that, as a rule, the temperatures of the insects are best studied when the thermometer is below 70° F., rather than when it is above that point, and the reason is as follows:

We know that in the use of thermo-electric apparatus, the degree of deflection of the galvanometer needle is proportional to the difference in temperature of the two junctions, and that no matter how great the degree of heat or cold may be, so long as both junctions are equally heated or chilled, no indication is afforded by the galvanometer.

Now if we place one of our junctions in the mouth of a man, exposed to an atmosphere of 40° F., we obtain a deflection of the needle proportional to the difference between the temperature of the mouth and that of the air, for the second junction which is exposed to the air of course acquires its temperature. One junction, therefore, is at 40° F., and the other at 99° F., a difference of 59°.

But suppose we wait till a day comes when the thermometer is at 90°.

The junction exposed to the air is now at 90° instead of at 40°, but the temperature of the mouth is still 99°, or at least has risen only to a very insignificant extent. Consequently the difference between our two junctions is now only 9°, instead of 59°, and the deflection of the needle is correspondingly less.

As the temperature of the air rises higher, the deflection of the needle grows less.

Finally, if we place the man in an oven heated to 130° or 140° F. temperature, that can be easily borne for a considerable length of time, we find that the galvanometer indicates that the man is very much cooler than the air.

Now it is true that the cold-blooded animals do not possess by any means that same power of maintaining a constant temperature as do the mammals and birds; still their temperatures can not be raised or depressed indefinitely, and there are consequently limits both ways at which they exhibit a certain resistance.

Suppose, for argument's sake, the higher of these two limits to be for a given class of insects about 90° F., the result would be that, as the temperature of the air approached this point, the body of the insect would gradually become cooler than the air, for the junction exposed to the air would have its temperature raised, while the insect would not warm up with equal rapidity. This case is parallel to that of a man exposed to a temperature gradually rising to 99° and above.

Suppose, on the other hand, the lower limit to be near 60° F., as the atmosphere cooled down to this point the insect would gradually become warmer than the air, for the junction out of its body would fall with the air, while the insect itself would not have its temperature depressed quite so rapidly. This case corresponds, therefore, to that of a man exposed to a temperature falling by degrees from 130° to 98° , or thereabouts.

We see, then, that the same insect examined, first, when the air is at 90° F., and second, when it is at 60° F., would, in the former instance, be cooler, and in the latter, warmer than the surrounding medium.

I was led to these conclusions from the fact that all the insects examined by me in the spring, from about the last of March to the middle of May, when the weather was comparatively cool, gave indications of a temperature decidedly higher than that of the air. But soon after hot weather set in, their temperatures, as a rule, conformed very closely with that of the air; and as the mercury rose still higher, the majority of them

were cooler than the atmosphere. A single series of observations will serve to show this change.

On a day towards the last of March, when the temperature of the air was 60° F., a large moth gave, with Becquerel's apparatus, a deflection of 40° galvanometric degrees* on the hot side.

In the early part of July, the air being at 75° F., an insect similar in all respects to the first, caused a deflection of only 3° on the hot side of the galvanometer.

During the first part of August, the air being at 84° F., another insect of the kind caused a deflection of 4° on the *cold side*.

A few days after this, a fourth insect was also cooler than the air, which had the same temperature as in the last instance, by six galvanometric degrees.

Similar results were obtained with other insects.

To verify these results, an attempt was made to examine the insects in atmospheres of different temperatures artificially prepared. Owing to unavoidable interruptions I did not succeed in carrying out my plans to the extent that I had hoped; but the few satisfactory results that were obtained were all in favor of the conclusions already given.

There is an important fact to be observed in this connection, viz., that in placing an animal in either a high or a low temperature, the extremely poor power of conduction of its tissues renders a change in its temperature slower than in the majority of inorganic bodies of equal size—of course this is also true of a dead animal.

When, therefore, we place an insect in a higher or lower temperature than that of the air, we must always allow a certain time to elapse before we make the connection with the galvanometer, in order that we may eliminate the above source of error.

With the approach of the cool weather of autumn my investigations were repeated, but unfortunately circumstances again prevented me from performing them upon a sufficiently extensive scale; still, so far as they went, the results were in the main confirmatory of those already given.

* Not to be confounded with thermometric degrees.

This was particularly true in the case of the grasshopper, an insect whose temperature I have carefully studied.

Of upwards of a hundred individuals examined during the hot weather of July and August, not a single one had a temperature above that of the air, and the great majority had a temperature below that point. When placed in air saturated with aqueous vapor, a portion had their temperatures raised as high as that of the air; but another still larger portion fell short of that point. In two or three cases the suspension of evaporation was followed by a rise of temperature on the part of the insect above that of the air; but these cases were so extremely few in number that I am inclined to attribute them to errors in experimenting.

During the last of September and the first of October twelve of these insects were examined. The temperature of the air was 70° F. and below. In every instance the suspension of evaporation caused the temperature of the insect to rise above that of the atmosphere, and in eight cases out of the twelve the insect exhibited a temperature slightly above that of the air before evaporation was stopped. In the remaining four cases the insect had the same temperature as the atmosphere.

I have thus given a synopsis of the principal part of my experiments on the temperature of the invertebrates, and trust that in so doing I have at least shown how much remains to be studied in this branch of physiology.

Hints about the Nature and Treatment of the Active Congestive Variety of Bright's Disease. By JOHN C. PETERS, M.D.

In a previous article we have had our attention turned to the effects of the retention of simple perspiratory fluid in the system. We have seen that the quantity of sweat which ought to be passed off daily may amount to 5 pounds as a maximum, to 1 $\frac{3}{4}$ pounds as a minimum, and to 2 $\frac{1}{2}$ pounds on an average. We have noted how an accumulation of this fluid, going on for days, weeks or months together, may induce general dropsy, *preceded* by an *œdematous* condition of the kidney; and have

cited in corroboration the case of a dropsical patient who was very thirsty, drank largely, and gained 11 pounds in weight in the course of three days from deficient elimination of the cutaneous and urinary secretions. But, in an acute or chronic check of perspiration there is much more than a simple non-elimination of water from the skin; for in one experiment Funke forced out nearly 7 ounces of sweat in one hour, containing $7\frac{1}{2}$ grains of urea, or in the proportion of 180 grains of urea in 24 hours; in another experiment the perspiration amounted to about 18 ounces per hour, containing nearly 10 grains of urea, or at the rate of nearly $\frac{1}{2}$ ounce of urea daily from the skin alone. Hence a contamination of the blood, with an excess of urea, can be induced by a simple check of perspiration.

Again, Schotten has demonstrated, with the greatest certainty, the presence of *formic* and acetic acids in normal human sweat, and very large quantities of formic acid have been obtained under Lehmann's own inspection; in fact, there is a great preponderance of formic acid, a far smaller quantity of acetic acid, and only traces of butyric or any other acid in natural perspiration.

Carbonate of ammonia occurs in alkaline sweat, especially in that collected from the arm-pits and feet. Lehmann infers, if he may form an opinion from the odor of different kinds of sweat, it is very probable that caproic and metacetic acids, which are closely allied to the butyric, are also often present in perspiration. In many diseases, especially such as are accompanied by an acute exanthematous eruption, there is often a singularly strong smell of metacetic acid. A sulphurous matter must be contained in sweat, for if fluid perspiration is kept in a closed glass we find that a considerable quantity of sulphide of ammonia is formed. But the substance which, next to water, occurs in the largest quantity in this fluid is the chloride of sodium. Finally, 412 cubic inches of carbonic acid gas, and about 200 of nitrogen gas are exhaled per day with the liquid secretion of the sudoriparous glands. The main object of perspiration is the elimination of an excess of water and of certain deleterious substances from the blood; still Peaslee is inclined to believe that mischief results from a check of perspiration, far more because carbonic acid and

nitrogen gases cease to be given off, than because urea, formic and acetic acids, chloride of sodium, sulphurous matter and ammonia are retained. However this may be, from $\frac{1}{4}$ to $\frac{1}{2}$ ounce of organic and volatile matter, and 38 grains of mineral substances should be excreted from the skin per day, and this does not happen in Bright's disease; for, the experience of almost every physician agrees with that of Goodfellow, that generally, in all stages of Bright's disease, there is an unusual dryness of the skin, and even during somewhat active exercise there is but little, if any, perspiration—the power of eliminating water seems not only impaired on the part of this great emunctory, but it is also almost certain that there is a diminished power of excreting some or most of the other constituents of the perspiration. Johnson says there is generally a dryness and harshness of the skin; the cuticular covering seems to be ill nourished and imperfectly formed; there is an unusual desquamation of the cuticular cells, and the exuviae are therefore more than usually great. He is often anxious to point out the fact that suppression of the cutaneous secretion is a frequent cause of such an alteration of the blood as produces disease of the kidneys. Not only is the skin dry, but there is difficulty in some cases of inducing free diaphoresis; now and then a patient will be found whose surface becomes very red under the influence of the hot-air bath; an artificial fever may be produced, but he will not perspire afterwards.

It is evident from the above that from either an acute or chronic suppression of perspiration, irritating substances enough may be retained in the blood to cause a blood poisoning but little less formidable in its effects than the inhalation of the paludal, or scarlatinal miasms; and, in consequence of these materials being conveyed to the kidneys more rapidly than they can be eliminated, they continue to accumulate in their blood, till the circulation through the kidneys is retarded and their functions impeded. It is possible that the excess of urea may narcotize, benumb or paralyze the renal nerves in some cases in the same way that it and opium do the brain, thus allowing passive congestion to arise; or some of the retained acids, especially the formic, may so irritate the kidneys as to produce active congestion, or subacute inflammation. How-

ever this may be, in the congestive form of Bright's disease, the kidneys are more or less turgid with blood, and are of a bright red or a dusky brown color; the surface is not unfrequently covered over with minute ecchymotic spots dependent upon the extravasation of blood into the tubes in their convoluted portions. This excessive congestion and extravasation of blood, by obstructing the tubes and interfering with the secretory function of the organ, is the chief source of danger. Professor Alonzo Clark says there is enlargement of the kidney, with no tendency to a white or light color. All the renal vessels are full, and in a thin section the malphigian bodies are seen often as bright-red spots, retaining the blood while it flows out of the veins. The kidney is loaded and distended with blood, but the characteristic exudations have not yet taken place. It is a state sometimes seen in the acute variety when death occurs early. Were it not for the symptoms, viz., the œdema, the pale complexion, the albumen and often the blood in the urine, the varying conditions of uræmia, etc., it might (and undoubtedly should) be considered as congestion, and nothing more. In some of Dr. Clark's cases the congestion was of a very marked character, so marked that where the kidney was left to drain, without being permitted to dry, (by being wrapt in oil silk,) the weight was diminished $1\frac{1}{2}$ ounces in one instance, in another 2, and in a third 3 ounces.

Watson describes this state of the kidney as one of *sanguine congestion*—the whole organ is gorged with blood, which drips freely from it when it is cut open. It is, in general, large, somewhat flabby, of a deep, dark red, even of a chocolate or purplish color, nearly uniformly diffused, except that the cut surface is usually diversified by still darker tuft-like spots, which have been ascertained to be the malphigian bodies, tinged with blood. This change from the natural appearance of the kidney is evidently of a recent kind; the symptoms are uneasiness, a dull pain in the loins, nausea and vomiting, a very scanty secretion of urine, which is sometimes palpably tinged with blood and always albuminous; occasionally complete suppression of urine, and in most cases sudden and general anasarca, or active, acute dropsy. The mischief done to the

kidney is the result of *extreme congestion*, and its actual consequences, viz., the oozing forth of the blood in substance, or of some of its constituents, into the interstitial textures, as well as into the excretory tubes of the kidney; those portions of the extravasated fluid which have no outlet of escape solidify, and thus obliterate the natural texture of the part they have invaded; the emergent veins of the kidney are often obstructed by firm clots of blood; blood casts, or fibrinous coagula moulded in and discharged from the urinary tubules and covered with blood disks, are often found in the urine. But none of these are necessarily products of inflammation, but merely of acute and severe congestion.

In fact, Dr. Robinson has clearly proved this experimentally, by placing a ligature on the renal vein of a rabbit; in consequence of which the urine became albuminous and bloody; he repeated this operation several times with almost uniform results. Frerichs has tested Dr. Robinson's experiments with a similar result, and, in addition, he observed, what Johnson admits might have been anticipated, that the urine which was albuminous and bloody, also contained *fibrinous casts* of the kidney tubes.

Braun's description is still more minute; he says, in the first stage, that of hyperæmia and commencing exudation, the surface of the kidney is smooth, the capsule easily removed, the plexus of veins on the surface dilated and full of dark blood; the cortical substance is brownish-red, soft, and friable; from the surface of a section there flows a sticky fluid, with which the parenchyma is infiltrated; the pyramidal masses are likewise hyperæmic, and the injection is striped; the mucous membrane of the pelvis and infundibula is swollen and covered with vascular arborescence, and they contain a bloody fluid. Apart from hyperæmia the finer structures of the kidney do not appear to be essentially injured.

Hemorrhagic effusions are very frequently observed, which sometimes take their rise from the glomeruli, sometimes from the vascular plexus of the tubuli uriniferi, sometimes from the veins on the surface of the cortical substance; the generally regular, round vesicles in the cortical substance originate in convoluted tubuli uriniferi being distended with blood; the

epithelium of the tubuli uriniferi is, in the first stage, not yet essentially altered, but may generally be distinguished by the ease with which it is separated; the tubuli uriniferi are filled with coagulated or fluid exudation, and appear as homogeneous, transparent cylinders, and sometimes contain blood corpuscles. These so-called fibrin cylinders occur less frequently in the chronic form of the disease.

It is in the decidedly congested kidney that we are most apt to find blood in the urine. Heywood Thompson found blood globules present in fifty-seven cases out of seventy-five of recent albuminuria. In well advanced cases the blood disappears, or at least it was not found in a single case out of ten. Dr. Clark says it is worthy of notice that very often in the beginning of Bright's disease the urine is sometimes found bloody and oftentimes of a smoky hue. The *smoky* urine is equivalent to the bloody urine, for when the matter that has produced the dark color is collected into a sediment it is found to be blood in some degree disintegrated; a portion is granular matter, and a portion blood globules. The granular matter is of a brown color under the microscope, and is supposed to be derived from the hematine of the blood converted into a substance known as hematinoidine. The only difference between bloody and smoky urine is, that the blood has met with some extractive matter which has converted its hematine into hematinoidine, almost always of the brown granular variety; and the same agent often causes shriveling and other changes in the appearance of the corpuscles. Dr. Clark considers the significance of smoky urine is the same as that of the bloody urine, so far as the condition of the kidney is concerned, both implying a congested state.

But, according to Dr. Henley, *urohæmatin* may be passed in large quantities, and the urine neither appear bloody or smoky; for it is sometimes largely discharged in the almost colorless urine of anæmia and chlorosis. An immense destruction of blood globules may take place in the body, and their debris be eliminated, so as to be invisible to the eye, until the application of an acid sets it free. Normally colored urine is hence often a treacherous guide to go by. The subjects may be well fed, but will lose flesh and strength without apparent cause; their urine may look natural, but their life's blood may be oozing

rapidly away by the kidneys. In some of these obscure cases the excess of urohæmatin is so great that when set free by an acid and taken up with ether, the mass, after standing, solidifies into a red-currant jelly-like mass, and may actually be cut with a knife. To detect urohæmatin, add one part of strong nitric acid to three parts of urine, bring it to the boiling point, and if there is much urohæmatin it will assume an *intensely red hue*. Or boil four ounces of urine with some nitric acid, to set all the coloring matter free; when cool, put the urine in a six-ounce bottle, with an ounce of ether, cork the bottle thoroughly, shake it well, and place it aside for twenty-four hours; at the end of that time the ounce of ether will sometimes be converted into a red, tremulous mass or jelly. In one case, the urine of a young man was almost colorless, like water, while in another it was of a deep-red color from hæmaturia; when strong hydrochloric acid was added to the colorless urine it rapidly assumed a port-wine *red* tint, while, when added to the bloody urine, the acid actually destroyed the color it already had; the pale urine had an excess of urohæmatin which was liberated by the acid; whereas, the red urine only contained a number of free, blood corpuscles, which became coagulated, and as the coloring matter in them is small, no sooner were their cell-walls destroyed, and the contained hæmato-globulin set free and precipitated, than the red color of the urine disappeared. Sometimes the urohæmatin may be in a free state, and the urine will be red before any acid is added, or pale-yellow in color, or brown. Then different acids may act differently; in one case, nitric, sulphuric, and muriatic acids, may give exactly the same results; in others, muriatic acid may turn the urine red, and nitric only make it yellow; in other cases, sulphuric acid may develop the color best. Urohæmatin patients may only have an irregular kind of dyspepsia, with occasional pain in the epigastrium; they may take plenty of food and be in easy circumstances, yet lose fifteen or twenty pounds of flesh per year, and grow weaker all the time. The urine in the worst form becomes neutral or alkaline; like albuminuria, it is apt to come on after diphtheria, pneumonia, and fever and ague, or after a fit of gout, and during convalescence from all severe diseases. But it is in the anemia and chlorosis

of both males and females, and the many obscure affections of that class, that it becomes a dangerous symptom, and may precede the developement of Bright's disease. It should be looked for in obstinate cases of so-called nervous debility. Dr. Clark lays some stress upon the occurrence of dyspepsia in Bright's disease; he thinks very few cases run their course without the occurrence of very marked dyspepsia, and that almost always of the flatulent variety, with a sense of oppression, and not unfrequently of fullness, or sometimes of emptiness across the epigastric region, and which will persist not unfrequently for weeks and months together. By the symptoms alone we can not distinguish the dyspepsia of Bright's disease from that arising from loss of urohæmatin; but an examination of the urine will clear up the matter at once.

Basham describes another form of discoloration of the urine by acid of a different significance from the above, viz., on the addition of nitric acid to urine *already coagulated* by heat, the albumen becomes altered in color, first assuming a *bluish green* and subsequently passing into a *greenish black*. This effect of nitric acid on the albuminous coagula results, he thinks, probably from the oxydizing agency of the nitric acid on the coloring matter of the urine, but adds that it has not yet been satisfactorily explained; but is known to occur most frequently in the acute form of Bright's disease, and is generally of very unfavorable significance; for his experience tells him that the development of this pigmentary condition, in combination with albumen in the urine is of the gravest import. It is always associated with a rapid and advanced state of renal degeneration, and has always been quickly followed by fatal results. He also quotes Lehmann, who says, as far as his experience goes, it is only when uræmic symptoms have or are about to manifest themselves, that this peculiarity of the urine is observable.

Epithelial cells and casts are also often found in the urine in simple but more or less severe and acute congestive Bright's disease. As long as the epithelial cells maintain their normal shape, size and appearance, congestion only is present; but as soon as they become abortive, or undergo some other morbid change, or are accompanied by compound cells, (Gluge's,) or other evidence of fatty or granular exudation, then either the

inflammatory form of Bright's disease is present, or has been superadded; or fatty or some other degeneration of the renal tissues has occurred.

It is evident from all that has been proven above, that the principal part of the treatment of the acute congestive variety of Bright's disease should be the restoration of the functions of the skin. For this purpose the *hot-air* bath is better than any vapor or water bath; but I have seen great benefit ensue from the addition of several or many ounces of carbonate of soda, or of a few ounces of spirits of ammonia to an ordinary warm bath, repeated daily, or several times a week. Persons who scarcely recollected ever having perspired before have done so after these baths; the skin has become soft and moist, and could be kept so if the patient was forced to wear flannel clothing from head to foot, with or without the superaddition of a complete suit of oil-silk; which latter often becomes very necessary in the coldest months of the year.

Dry or wet cups to the loins are often useful, but perhaps not more so than stimulating applications, such as mustard water, made with two handfuls of mustard tied in a muslin bag, placed in hot water, and squeezed with the hand, until all the strength of the mustard is extracted; a thick, broad flannel or towel, long enough to reach entirely around the loins and abdomen, may be wet with this infusion, and worn, covered with oil-silk.

Of the remedies for internal congestion, the most efficient is a combination of tincture of root of aconite, tincture of veratrum viride, tincture of digitalis, and tincture of colchicum. All these medicines tend to equalize the circulation, while aconite is a specific antiphlogistic diaphoretic, digitalis a specific antiphlogistic diuretic, and colchicum unloads the biliary and intestinal capillaries; finally, all these remedies eliminate urea from the blood.

A New "Inhaler" for Sulphuric Ether. By F. D. LENTE, M.D., of Cold Spring, N. Y.

With a view to facilitate the induction of anæsthesia by sulphuric ether, and to overcome some of the objections to its

use, I have had constructed, at Otto & Reynders', No. 64 Chatham street, a very simple and cheap apparatus, of which I propose to give a brief description, premising that one of the principal causes of delay in the induction of anæsthesia by ether, and a consequent waste of the material in the hands of most operators, has been the necessity of withdrawing the apparatus from the face, at intervals, in order to replenish the ether; and, I may add, the unnecessary deliberation with which this is generally done, thus allowing the respiration of pure air to a greater or less extent. A principal aim, therefore, of the new contrivance is to obviate entirely this necessity.

It consists, first, of a light wire, helmet-shaped framework, so formed, at the base, as to fit over the nose and chin, but not to cover the eyes. At the apex of the cone is a male screw; over this framework is a cone of double flannel—a hole at its apex allowing it to slip over the screw. Over this is screwed an arrangement consisting of a short tube half an inch in diameter, and branching into three small tubes, so arranged, when adjusted, as to embrace and open upon the flannel cone at equal distances from each other. Over the tube is slipped a cone of impervious material, not easily acted on by the ether, as enameled leather, with the enamel outside. This cone is lined with tin-foil, so as to prevent the absorption of the ether by the leather. This cone is kept *in situ* by a nut screwing over the tube; over the extremity of the latter is drawn the end of a vulcanized India-rubber tube, the other extremity of which is to be slipped over the neck of a suitable bottle containing four or five ounces of ether, as soon as the inhaler is required. Upon placing the cone over the patient's face, and inverting the bottle, the ether runs very rapidly through the three tubes and distributes itself equally to all parts of the flannel cone, whether the inhaler be held vertically or horizontally.

In using the inhaler, it is first to be accurately adjusted to the face so as to exclude, as far as possible, all air.* The

* By placing the inhaler over his own face as firmly as possible, without ether, and breathing through it, the operator may convince himself that there is no danger of suffocating the patient by cutting off a due admixture of atmospheric air.

bottle is then to be inverted and about a couple of drachms of ether allowed to run into the flannel; after the patient has respired a few seconds and become a little accustomed to it, about three drachms more are to be supplied, and repeated about every half minute; if a superabundance is allowed to run in it will be found running down the neck of the patient at the bottom of the flannel cone. Experience soon teaches just how much is needed from time to time. In case the patient struggles, the bottle may be allowed to hang by the India-rubber tube, and both hands used to keep the inhaler closely applied to the face, which is very important; or, when he has become quiet, one hand may be employed in assisting in the operation, if necessary, and within reach. This is of no little importance in the country where assistants are usually limited; when we are operating by candle-light it is also a great convenience to have the ether bottle so arranged as to preclude all danger of explosion. At the first public trial of the inhaler, in a case of trephining, for epilepsy, at the New York Hospital, recently, the patient was completely insensible in a minute and three-quarters, and with about an ounce and a half of ether; but a longer time and a somewhat larger quantity will probably be an average. As soon as the operation is over, the inhaler should be taken apart, and the flannel washed and dried well before it is put together again.

It is proper to add that my friend, Prof. Thomas, a year ago suggested to Mr. Tiemann the construction of a cone similar to this, and has used it himself. In his apparatus, however, there is no tubular arrangement for supplying the ether to all parts of the cone at once, and the bottle is not attached to the inhaler.

*Case of Constipation of nine weeks and two days Duration
Cured by the External Application of an Aloetic Preparation.*
By HERBERT M. HOWE, M.D., Episcopal Hospital, Philadelphia, Pa.

Lizzie F—, aged nineteen years, single, admitted into the Medical Ward of the Hospital, October 26th, 1865, as a case

of phthisis. Lived on a farm in Columbia Co., Pa. Her work consisted in performing various duties about the farm, such as attending to the dairy, and, when the weather would permit, she found her employment in the fields. She never was very strong, though she came of healthy parents. Her health had been perfectly good until the last Monday in August, when she took a warm bath; having finished this, she turned on the cold "shower," and stood under it. From that time she became sickly; her menses, which were formerly regular, stopped—cough began, accompanied by profuse expectoration, and her appetite left her. On the Thursday following, which was the first day of September, she had a healthy evacuation of her bowels. Since that time, September, till November 4th, she had not had her bowels opened once. This statement, at first so seemingly improbable, is rendered less so by the following facts: she has eaten very little during the whole time, and she has vomited very frequently, at times every other day, while at others several times during the twenty-four hours. The matter vomited has been digested, generally yellowish, though sometimes greenish, and always intensely acid. Under these circumstances, as might be expected, she lost flesh rapidly, and is now quite emaciated. Her chronic constipation she attributes to having eaten a great many chestnuts, on several different occasions, at about the time of her commencing illness. Since the 1st of September she has been a great sufferer from headaches, at first so severe as almost to set her frantic. There has been no pain in the abdomen or stomach, and no wind. When she first made known her constipated condition and told of its duration, I discredited her statement; but on subsequent further questioning I find her story to be so accurate and complete, and the girl to be one having very good abilities, that I do not doubt the truth of what she represents. At first I gave her three pil. cath. comp. These were taken on the morning of November 3d. Learning that there had been no passage procured by the following morning, she had given her

R.—Oleum Tiglii, gtts ij.

Panis - - - q. s.

M.—Ft. mass. et in pil. iv. div.

One of these were to be taken every two hours, accompanied by frequent enemata of soap and water. The first was given at 11 o'clock, A.M., of the 4th. This treatment produced but a miserable apology for an evacuation, probably no more than the injection could reach and remove mechanically. The following day, finding that she had had no relief, I directed that a large cloth be saturated in red wine of aloes, and placed over her abdomen. This was done at 10 o'clock, A.M., and kept on until 3.30, P.M., when she had a copious stool, followed at five o'clock by another. She reported herself as feeling like a different person, so great was her relief.

One could hardly suppose that the stools were caused by the Croton oil given on the previous day, especially when we remember its very prompt cathartic action. Prof. Wood, in his work on Therapeutics, says of this medicine: "As a purgative, it operates with great rapidity, often in an hour or less;" and he is of the opinion that "there can be but little doubt that its effect is due to its direct irritant action on the mucous membrane." Any irritation that could have been produced by giving the medicine in pills, each containing only half a drop, and that, too, given at intervals of two hours, would have passed off during the following twenty-four hours. It is well known that aloes sprinkled over the surface of an ulcer will purge: why should not a similar effect be produced by the quantity that the skin would absorb when the medicine is applied in a liquid form over an extensive surface? And in many cases may this not be a convenient mode of emptying the alimentary canal?

Removal of a Large "Floating Cartilage" from the Knee-Joint, with Successful Result. By JAMES WILSON, Licentiate of the Royal College of Surgeons in Ireland, etc.; late Staff Surgeon U. S. A.

In the practice of our profession there are probably few subjects of greater interest and of more vital importance to the reputation of surgery, than the operative treatment of moveable bodies in the cavities of the larger joints, and especially of the

knee. Obscurity surrounds their pathology, and there is no operation apparently so trivial that the surgeon will be more slow to recommend and more solicitous as to the result. Be the intruder small or large, single or multiple, fibrous, cartilaginous or calcareous, the result will be to an extent the same; an intolerable annoyance at the best, producing irritation, inflammation, and effusion into the synovial cavity, if not organic disease itself, and at times giving rise to such excruciating and insufferable agony that the patient is finally compelled to submit to whatever operation the surgeon may recommend as being best adapted to the exigency of the case.

Unfortunately, there is no method of procedure by which we can always avoid the terrible consequences incident to wounds penetrating the larger synovial cavities; but since Syme and Goyrand suggested the applicability of subcutaneous surgery to the removal of these bodies, the risk has been so materially diminished that in most instances we are justified in entertaining a reasonable expectation of the entire success of a properly executed operation.

Such was the method which I considered the most favorable to have recourse to in the following case, and I think the result is worthy of record, not only as corroborative of the testimony already adduced in favor of this peculiar operation, but as illustrative of its adaptability to the removal of foreign bodies of the largest possible size.

Private Finnerty, 99th Reg. N. Y. Vols., aged 24, a florid and healthy looking young man, presented himself at "sick call," with well marked symptoms of subacute inflammation of the left knee-joint. The history of his case is briefly as follows: A little over two years previously, while doing guard duty at the "Rip Raps," he fell from a considerable height, striking the outer side of the affected knee against a rock, and injuring it very severely. He was admitted into hospital, where he remained for a month under the care of Asst. Surgeon Rodgers, (now 6th Regular Cavalry). No fracture was detected, and he was ultimately discharged from hospital with an apparently sound knee-joint. Four or five months subsequent to his being returned to duty, he detected a small "tumor" over the upper and anterior surface of the inner condyle, which, he states, was

about the size of a small bean. It was immoveable, tender when manipulated, but did not impede locomotion or prevent his doing duty. About three months afterwards, while on the march from "Deep Creek" to Suffolk, he again injured the knee by attempting to jump over a fallen tree; he distinctly felt something "snap" in the joint, and was so lame as to be unable to do duty for a week afterwards. When he came under my charge the tension from synovial effusion was so great as to render the detection of the intruder for a time impossible. Absolute rest, combined with ordinary antiphlogistic treatment, however, soon caused sufficient reduction of the swelling to enable me to detect the offending body. It was then situated at the upper and outer side of the joint, and seemed to be about the size of an ordinary patella. It was exceedingly moveable, and could be made to glide from one side of the joint to the other, under the tendon of the quadriceps extensor, with the greatest ease. The pain, however, produced by even the gentlest manipulation, was excruciating, and accompanied with a sensation of nausea and faintness. After a protracted and careful preparatory course of treatment, I removed the "cartilage," by the subcutaneous operation, from the cavity of the joint to a position external to the capsule, sealing up the puncture made by the tenetome with a small patch of lint dipped in collodian. No untoward symptom followed this first and most important step of the operation, and at the expiration of eight days, I removed the cartilage by a direct incision. The integumentary wound healed rapidly, and the subsequent progress of the case was as satisfactory as could possibly be desired.

Finnerty was soon returned to duty and served the remaining year of his enlistment without experiencing the slightest annoyance from his knee, even after the most protracted exertion and fatigue.

The cartilage, when extracted, proved to be of very unusual size; it was of an irregularly oval outline, convex on one side, concave on the other, osseous in structure, and exceedingly rough and irregular on all its surfaces. It measured one and a half inches in its longest, and one and one-quarter inches in shortest diameter; it varied from one-half to one-quarter of an

inch in thickness, and weighed seventy-two grains. Attached by a slender pedicle to one edge was another and smaller concretion of an almost similar structure, and about the size and shape of a small buckshot, but so rough and irregular as to be almost jagged. The presence of this smaller body was not detected till after the operation.

In the first volume of the "Principles of Surgery," by James Miller, Professor of Surgery in the University of Edinburg, the author suggested an operation by "*transfixion*," which, theoretically considered, seemed as feasible and safe as it was ingenious. The operation was based on the idea that the foreign body might be permanently secured in a safe part by transfixion with one or more needles, which were allowed to remain "till sufficient excitement is induced for fibrinous exudation of a plastic kind along the track of the needle, and thereby the previously moveable substance is fixedly incorporated with the parts."

At a later date a case was brought before the Medico-Chirurgical Society of Edinburg, by Professor Miller, for the purpose of making a *recantation* of this principle which he had inculcated in his work on surgery. It was briefly as follows:

A young man was admitted, March 7, 1854, with a loose cartilage in his knee-joint. After the most careful preparatory treatment two needles were inserted over the external condyle, and the cartilage was transfixed—not, however, without great difficulty.

March 18th. Needles withdrawn, having been inserted for one week; no unfavorable symptoms.

March 24th. The cartilage suddenly slipped away from its position; it was again seized and transfixed, but only with one needle.

April 14th. Needle removed and patient got up.

May 2. The cartilage again slipped from its place into the cavity of the joint while the patient was pulling on his boot.

May 3. Professor Miller again transfixed the cartilage, but, in two more days violent inflammation set in, and the unfortunate subject on which this theory had for the first time been reduced to practice left the hospital minus his leg.

A Case of Epithelial Cancer of the left Cheek and the adjacent portion of the Lips. Removal of the entire substance of the Cheek, and successful closure of the opening by a Plastic Operation. By J. C. HUTCHISON, M.D., Professor of Surgery, Long Island College Hospital, Surgeon to Brooklyn City Hospital, etc. Reported by J. C. GOODRIDGE, Jr.

Mary Sullivan, aged 54. Born in Ireland, at present residing at No. — Baxter street, New York, applied for treatment April 1st, 1864. She stated that two years ago she pulled out a long hair, which grew from the centre of the left cheek, when a small quantity of matter exuded. The skin soon began to ulcerate at this point. The ulceration gradually increased in size until it extended over a surface measuring two inches in its transverse by two inches and a quarter in its vertical diameters.

She had been accustomed to keep her face tied up with a handkerchief, (to conceal its deformity,) in consequence of which the motions of the lower jaw were limited from contraction of the masseter muscles. Her general health is good; she suffers no pain from the diseased part. There appears to be no hereditary tendency to disease.

On the 6th of April the following operation was performed, in presence of the Medical Class at the Long Island College Hospital.

The entire diseased portions were removed by an incision circumscribing it, and extending entirely through the soft parts, making an opening into the cavity of the mouth, two and a half inches on both vertical and transverse diameters. Several ligatures were required to control the hemorrhage.

The soft parts around the opening were detached with a scalpel from the upper and lower jaws, and a T-shaped incision above and an L-shaped incision made below the wound. The vertical portion of each incision was about three-fourths of an inch in length, and opened into the circular wound. The four flaps thus made were brought together, and the lips drawn back to fill the triangular space that remained. The parts were fastened by the hair-lip suture.

There was no great amount of tension in any direction. She

received the most nourishing diet in a liquid form. Warm water dressings were applied to the wounds.

April 8. The needles were drawn out, the threads remaining. On the following day the threads near the mouth came off.

April 10. Suppuration took place at the junction of the flaps, and they separated in about two-thirds of their extent.

April 12. The flaps were brought together by silver wire sutures, which soon sloughed out. The flaps were then kept approximated by adhesive straps.

She had an attack of erysipelas in the face and head for about five days. The inflamed parts were circumscribed by tinc. iodide, and washed with lotio. plumbi. acetatin, and quiniæ and wine given her.

From that she gradually improved in general health, and the flaps united throughout their whole extent, with the exception of a space two-thirds of an inch in size at the angle of the mouth. She was discharged from the hospital May 12.

Prof. Hutchison expected to close the opening which increased the width of the mouth by about two-thirds of an inch by a subsequent operation; but when next seen, after four months had elapsed, it had closed spontaneously.

Now there are no marks of the operation, except the cicatrices that partially show the line of the incisions.

She opens her mouth without difficulty. There is no doubling of saliva; her general health is good, and she expresses great satisfaction in being thus relieved of her unsightly disease.

A microscopic examination of the excised parts, by Dr. S. Fleet Spier, showed it to be epitheliel cancer.

When we consider the obstacles which tended to prevent union in this case, viz., the free hemorrhage requiring the application of several ligatures, the suppuration and separation of the flaps after the removal of the pin, the attack of erysipelas, the second separation of the flaps, after being drawn together, by silver wire sutures, and the necessity of approximating them a third time by adhesive straps which were being constantly loosened by her food and discharges from the mouth, the result of the operation was far more successful than could be anticipated, and inspires us with increased confidence and respect for the "*Vis Medicatrix Naturæ*."

PROCEEDINGS OF SOCIETIES.

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, October 4th, 1865.

Dr. ALFRED UNDERHILL, First Vice-President, in the Chair.

A communication from the Academy of Medicine of Barcelona was read, expressive of the great loss sustained by the medical profession throughout the world in the death of Dr. Valentine Mott.

DISLOCATION OF THE HEAD OF THE FEMUR INTO THE ISCHIATIC NOTCH.

Dr. SAYRE presented the following case:

I wish, Mr. President, to present to you a case of rather unusual interest—a case of spontaneous luxation of the femur into the ischiatic notch, of eight months' standing, reduced by manipulation, and supported by an artificial contrivance, by means of which the person is enabled to walk. It is the case of Col. Wm. A. Bullitt, aged twenty-four, of the 3d Kentucky Infantry. He was wounded May 9, 1864, at Rocky Face Ridge, Georgia, in two places—first at a point five inches below the anterior sup. spine of the ilium of the left side, and directly in front; this ball did not emerge. The second shot passed through the right chest obliquely, from above downwards and before backwards, and was received while in the act of rising. The first brought him to his knees; the second threw him violently, rolling him over. He arose immediately and walked half a mile to the rear, and was then carried on a stretcher some distance further, when he met a surgeon who examined the wound in the right side, and, considering it mortal, did not dress it. He was then transported five miles further to the hospital, arriving there four hours after having been wounded. Here another surgeon saw him, and administered some morphine. The next day he was sent to Chattanooga, arriving there at 5 P.M., twenty-four hours after the receipt of his wounds. There he was attended by the surgeon in charge, and his wounds dressed for the first time. On the 9th day, May 18th, erysipelas commenced at the wound in the chest, and rapidly extended over the whole body resulting in numerous abscesses in the subcutaneous cellular tissue.

On the 5th of June he was removed to Louisville. The erysipelas was succeeded by dysentery, which was again succeeded by another attack of erysipelas, which extended over the whole body. About the 1st of August he began to suffer pain in the left iliac fossa, which was at first paroxysmal, and continued a month before the abscess

which occasioned it was discovered. The abscess was opened above Poupart's ligament, and a large quantity of pus, estimated by the surgeons present at half a gallon, evacuated. When the pain commenced, for the purpose of relieving it he gradually drew up his knee, at first only during the paroxysm, which came on about 5 p.m. every day; afterwards he kept the knee closely approximated to the chest, and the attempt to straighten it caused severe pain. The abscess continued to discharge for three months. About the 20th of October he began to lie over on his right side, (previous to this, since the formation of the abscess, he had lain constantly upon his back,) and shortly after, upon turning over from his back to his side, he felt the head of the femur slip from its place, causing an excruciating pain. This occurred almost daily for two weeks, and the attention of the attending surgeon was drawn to it. About the 1st of November a protuberance under the gluteal muscles was noticed by the patient, and the attention of the surgeon having again been called to it, he recognized a dislocation. A few days after, Col. Coolidge, Medical Inspector U. S. A., Dr. Goldsmith, U. S. A., Dr. McDermott and several other gentlemen attempted reduction, with the aid of ether. Their efforts were not successful. About this time the abscess closed.

In the latter part of February, 1865, four months after dislocation, another attempt to reduce it was made by Prof. Cook, Dr. Foree, Dr. Cox, Dr. Galt, and Dr. Garvin, all of Louisville, which was also unsuccessful. A few weeks after he got out of bed and began to move about on crutches. He then consulted Prof. Miller and Dr. Bullitt, of Louisville, who advised him to come to New York and place himself under my care. He arrived in New York the 24th of May, and by the kindness of Col. Sloan was accommodated at the Central Park Hospital. I saw him in June, and found him in the following condition: his general health was good, and he was able to go about on crutches very well. His left thigh was flexed at nearly a right angle with the pelvis, and strongly adducted across the right thigh about the junction of the middle and upper third. The leg was flexed upon the thigh at about a right angle. As he lay upon his back a plumb-line dropped from the left knee fell four inches from the outer side of the right thigh. The genital organs were very much compressed, and the difficulty of urinating was a very serious annoyance. The whole limb was much colder than the other, bathed with perspiration, and very much shrunken in size. There was some slight perceptible motion of the hip, and only slight extension at the knee, even by considerable force, which was always attended by great pain.

Reduced on the 20th of June, 1865, eight months after dislocation.

The patient was placed thoroughly under the influence of chloroform. Four or five blankets were spread upon the floor as a solid basis on which to work. Removing my boots, and placing my heels on the crest of each ilium for the purpose of fixing the pelvis, I succeeded in giving mobility to the hip; and placing my finger high up in the rectum, I discovered the head of the bone moving in the ischiatic notch. This, of course, settled its location. Reduction from that position, by extension across the pelvis, was out of the question, and reduction by manipulation, according to Reid's method, was interfered with very seriously by long continued contraction of the adductor muscles, and at the same time adhesions had occurred subsequent to the extensive suppuration which had taken place. Whether I could succeed in reducing it was questionable. I told this to the gentlemen present; but we all regarded it as a case worthy of the attempt. If I were to succeed, very well; if not, I should leave him for several weeks, until all traces of inflammation, which would result from such violence, should have subsided, and then make subcutaneous section of the tendons of the contracted muscles, and in time restore the limb to parallelism with the other, leaving the head of the bone in its abnormal location. Drs. Sloan and Hamilton agreed to this plan of procedure. The patient having been very satisfactorily anæsthetized by Dr. Bradford, I succeeded, after considerable effort, in restoring the thigh to its normal position, after the method so well described by Dr. Reid, of Rochester. The thighs were now parallel, but the flexion of the leg upon the thigh by the contraction of the hamstring muscles we were not able to overcome. I could not straighten the leg thoroughly and completely; but the fact that it became very much nearer straight than it was before induced me to resort to a continued extension by means of a weight and pulley.

He was placed on the bed, and adhesive plaster applied below the knee in order to extend the leg. Extension was also made by adhesive plaster on the thigh, at an angle four or five degrees higher. Another weight and pulley was now applied to a band surrounding the knee, in such a manner as to antagonize the adductors of the thigh. The foot of the bed being raised a few inches, the weight of his body became a counter-extending force. The hip was now kept constantly wet by means of an irrigating apparatus. The first two nights, morphine in considerable quantity was administered. He had no constitutional disturbance, his health being perfectly good; you

scarcely would have observed that any serious operation had been performed, though the operation was, of course, one of great severity.

His leg came down, not only to the length of the other, but when his knee was extended perfectly straight it was a trifle longer; but upon his getting up to bear his weight upon it, it immediately receded, and became some inches shorter than the other, there being no acetabulum for the head to rest in. Eight months having elapsed since the dislocation, the acetabulum had become obliterated in a great measure by inflammatory deposits. The trochanter major of the luxated side was some lines further from the centre of the pubis than the other, showing that the head of the bone, although opposite the acetabulum, was not in it.

In order to overcome this, and to permit the patient to have the advantage of out-door exercise, I first took a plaster cast of his hip and thigh, in the improved position. The object of taking this cast was, by it to construct such an instrument as would enable him to bear his weight upon the limb without permitting the head of the femur to ride by the acetabulum.

This instrument consists of a crutch which goes under the perineum and around the gluteo-femoral fold, accurately moulded on and embracing the buttock. This crutch is stuffed and padded, and forms an easy seat. Directly opposite the tuberosity of the ischium is a socket-joint, and from it emerges a steel splint, or rod, capable of being extended or shortened, which terminates opposite the calf of the leg, in a fork. Each prong of the fork is continued down and around the leg, terminating opposite the ankle-joint, on each side, in a little ball or knob. A neatly fitting, comfortable shoe, which he now wears, was then made with a thick buckskin tongue, to prevent injurious pressure of the shoe-laces. On the sole, or rather in the shank of the shoe, is bolted a steel-plate, with rods running up each side and terminating opposite the ankle-joint, in two little cups, destined to receive the balls of the splint.

This instrument was applied in the following manner: The shoe was put on and laced up, the crutch then inserted under the perineum and tuber ischii, and buckled around the thigh by straps provided for the purpose. The prongs of the fork were then placed in the sockets, and extension made by means of the ratchet and key. A knee-cap was then buckled on, bringing the knee back towards the splint. With this apparatus his thigh can be extended to the length of its fellow, and he is enabled to stand with comfort, to bear his weight on the limb, and to walk tolerably well. This was put on twenty-nine days

after the operation, and for two or three days he walked about easily, and it was almost impossible to persuade him to go to bed.

He unfortunately had from this time, on account of the shoe being too tight, an inflammation of the toe-nail, which eventually required its entire removal. About this time he was removed to David's Island, the Central Park Hospital being closed up by orders from Washington.

He was confined to bed for a week or two after this operation, but has now recovered entirely from it, and intends leaving for Kentucky immediately. He is able at this time to cross his legs, is increasing the mobility of his limb, and can put his leg up a step to go up stairs.

I would state, Mr. President, that I looked over Gross's Surgery, and find that he gives three months as the longest period at the end of which an attempt at reduction of the humerus would be justifiable, and eight weeks for the femur. He also lays great stress upon the immense importance, the absolute necessity, in fact, as he states it, of a course of preparatory treatment before attempting the reduction of an ancient dislocation—that it is only in persons of lax fibre, and in old patients, that it should be attempted at all. Now, in very old persons there would be great danger of producing a fracture instead of a reduction. He says the system from the first must be prepared by bleeding, which is to be frequently repeated, and by the constant use of mercury to the point of ptyalism; that low diet should be used, movements of the limb every day, once in twenty-four hours at first, for a fortnight, and afterwards once in twelve hours. I do not know that this is the proper time and place to discuss this question, but I may state that in this particular instance I adopted the plan which I have always adopted since I have been engaged in the practice of surgery: to consider the patient best prepared for an operation just as soon as the operation becomes necessary, or is discovered to be necessary. I believe the preparing of a patient for an operation, by any kind of treatment, to be attended by so much anxiety on his part as to more than negative any benefit which the preparation might in other respects confer. The anxiety incident to the knowledge that he is being prepared for a great and critical operation must necessarily be wearing upon the system. In the case before you there was no preparation made, except simply diagnosticating the case; and my position is, that the best time to perform such an operation is when you find it is necessary to be done.

Dr. BUCK—It was not by violence that this dislocation took place; and there was a gradual process resulting from the peculiar position

which the limb had then occupied for a long time, by which pressure was exerted upon the capsular ligament in the direction in which the bone escaped from its socket, the capsule having become relaxed and extended. Such an occurrence has taken place, and I recall one instance which comes directly to my mind, where it resulted to a patient long confined to the bed from some cause—gradual relaxation of the capsular ligament in the direction in which the bone escaped; so that in this dislocation the head of the bone had been displaced from its socket, but had not escaped from the capsular ligament; and I don't think we are warranted in deciding that in a dislocation occurring from violence, in which a rent is made in the capsule and the head-escapes, that reduction is possible after so long an interval. I conceive that one great obstacle to replacement would be the impossibility of returning the head of the bone through this rent, which this long interval had closed up. The head of the bone might be brought to correspond with the acetabulum—might be brought, by the application of sufficient force, from its abnormal position; but the interposition of the capsular ligament would then be found to be the great obstacle to be overcome in effecting the replacement. The case is a very interesting one, sir, but I do not know whether or not I am prepared to agree with Dr. Sayre in his conclusion in regard to the acetabulum being filled by *deposit* within that period of time. That is a point which I should not consider as demonstrated.

Dr. SAYRE.—It can not be demonstrated till the person dies, and we know by positive examination.

Dr. BUCK.—It is a question which is certainly open for discussion. I would remark that a case has occurred in this city, within a recent period, in which a congenital dislocation of the femur has been reduced after a period of, I think, several years. It was somewhat analogous to this in the fact that the head of the bone had not escaped from the capsule, though dislocated from the acetabulum. The cure was effected by a process of gradual extension, in the hands of Dr. Davis of this city.

Dr. SAYRE.—I would like to ask if a congenital dislocation has ever occurred; I don't mean at the time of birth by the doctor's manipulations—I mean a congenital dislocation occurring *in utero*?

Dr. BUCK.—That was not my meaning; I meant existing very early in infancy.

Dr. SAYRE.—That is what I mean, too; and we ought to understand definitely about this congenital dislocation of the femur. We all know the formation of the femur and the construction of the acetabulum,

being made of the three distinct bones which in early life are not united; at a certain stage of development there is an arrest, and this arrest of development prevents the formation of the acetabulum; we ought not to call that a congenital displacement or a congenital luxation. You must have an acetabulum in which the head is fitted in order to luxate it. I do not believe that any gymnastic exercise of a child *in utero* can be carried to such an extent as to displace a healthy femur from a completely finished acetabulum; and, therefore, this whole subject of congenital dislocations should be looked at in an entirely new light.

Dr. KRACKOWIZER.—I rise, Mr. President, simply to speak of a case of spontaneous luxation of the femur, which reduction I effected after about five months had elapsed. The subject of this accident was a girl about sixteen or seventeen years of age. Early in April she was attacked with articular rheumatism invading several joints, upon her recovery from which the right thigh was found to be very much out of the usual place, and it was impossible to move it. I saw her in October following, and as I did not think manipulation would be sufficient to reduce the head, I resorted to the usual contrivances for fixing the pelvis, and counter-extension and extension; but, although I heard and all heard the crackling and giving way of adhesions, yet the head was not returned. I should mention that I used no pulleys—had the assistance of nothing but the hands of attendants. After this method had failed, I resorted to Fisher's or Gross's method, and, not with a clear snap, but with a sort of concussion, which could be distinctly heard, the head returned to its natural place. At that time I was not familiar with the method of *elastic* extension, which is so valuable in treating displacements of joints, but merely held both legs in the usual manner together, and in two or three weeks the girl could rise and walk about. Whether in this case the luxation occurred suddenly, or by degrees, can not be determined. The observation of the patient and attendants is, I think, entitled to great weight in deciding this question. In most cases of spontaneous luxation that I remember to have heard described, the dislocation occurred suddenly, a fact which would weigh in favor of the idea that spontaneous luxations are not materially different from rheumatic ones. The theory that luxation occurs in consequence of the capsule being very much dilated, and the head slipping out of the socket, and yet being within the capsule, is, in my opinion, very largely a speculative one. I think the same difficulties are in the way in the reduction of a spontaneous as a traumatic luxation, with, perhaps, this difference: if a rent

in the capsule has been a more or less surrounded one, the probability is that the head of the femur, having worked against a certain point of the capsule, has produced a sort of laceration, so that the head slips out of the hole, which then can not contract so fast as a slit produced in a traumatic luxation; and, through a larger loss of substance, a longer time is required to reduce the luxated head than if it had slipped out through a laceration in the capsule.

Dr. POST.—During the past week I had occasion to reduce a dislocation of the shoulder-joint, a case which is somewhat remarkable from its great antiquity, and for its being reduced without the aid of instruments. The patient was a laboring man, upwards of forty years of age, who, in the beginning of last May, fell and dislocated his shoulder, the head of the bone being thrown under the pectoral muscle. He was told, at one of the dispensaries, that it was only a sprain, and the physician who had him in charge told him he could do nothing more for him, and no further treatment was adopted until he applied to me last week. I found unequivocal signs of the location of the head of the bone under the pectoral muscle. I made the attempt to reduce the dislocation without much expectation of success, not being provided with instruments at the time. The patient was first placed upon a bed and brought under the influence of ether to a state of anæsthesia. I made an extension and a counter-extension, and afterwards had two persons employed in making an extension, and two in making a counter-extension. These were kept up, I think, a little more than half an hour, during which time I made frequent manipulations without success. At the end of that time, I directed the extending and counter-extending forces to be suddenly suspended, when with slight manipulation the reduction was readily effected.

The time that elapsed from the dislocation to the reduction was 147 days; and I am not aware that there is any other instance where the dislocation of a shoulder has been reduced after so long an interval without the aid of mechanical contrivances.

Dr. SAYRE.—I would simply state, in regard to the luxation in the ischiatic notch, and in confirmation of the views of Dr. Krakowizer, that the head of the bone would drive through the capsule, and therefore a rent have occurred, from the fact that the head of the bone was distinctly detected by Dr. Hamilton, myself and others; and it is impossible to place the head of the bone in that position without rupturing the capsule. I think that has been proven by Dr. Bigelow, of Boston, beyond the possibility of a question. In taking the dead subject, as I have frequently done, and luxating the limb in different posi-

tions and dissecting it afterwards, it will be found that you can luxate the limbs upon the dorsum of the ilium, upon the pubes, and in the *femoral* ring, without tearing the capsule; but, so far as I have tried, and Dr. Bigelow confirms the opinion, you can not luxate the head of the femur into the ischiatic notch without tearing the capsule, although the other luxations may occur without it. And the fact that this bone was distinctly felt by Drs. Hamilton, Sloan and myself, in the ischiatic notch, is an evidence that the capsule in this particular case was ruptured—I suppose by laceration from constant pressure.

GUN-SHOT FRACTURES OF THE THIGH.

Dr. Post, by request, read the following abstract of a report, prepared for the information of the Sanitary Commission, on the subject of gun-shot fractures of the thigh.

Gun-shot fractures of the thigh involve several important practical questions, with reference to their prognosis and their treatment. Many of these injuries are complicated with lesions of important blood-vessels leading to the speedy death of the patient from hemorrhage, on inducing gangrene of the limb, followed by a fatal result within a few days after the injury. In other cases, the ball, in its passage through the limb, comes into contact with one of the principal arteries, contusing its coats, and causing it to slough within one to three weeks after the injury, giving rise to secondary hemorrhage, which often terminates the life of the patient.

In many cases, the bone is greatly comminuted, and the fragments are driven with great force into the adjacent tissues, giving rise to excessive and exhausting suppuration, proving fatal to the patient by the long continued drain upon the vital powers, or terminating life more speedily by inducing pyæmia. When the injury involves either of the articular extremities of the bone, communicating with the hip or knee-joint, the inflammation which follows is more severe and the suppuration more profuse, there is a greater degree of constitutional irritation, and the prognosis is more unfavorable than when the fracture is confined within the limits of the shaft of the bone.

When gun-shot fractures of the thigh occur in battle, and it becomes necessary to transport the patient to a considerable distance, especially over rough roads and with imperfect facilities for transportation, the danger of the case is greatly aggravated. The danger is also much increased, when it becomes necessary to crowd a large number of wounded persons in narrow or ill ventilated apartments. The want of proper food and medicines, and of comfortable beds and bedding, and

of suitable dressings for the wounds, and of supports for the fractured limbs, increases the fatal tendency of the injury. Dr. Crosby, of Columbia College Hospital, in Washington, informed me that numerous cases of gun-shot fracture of the thigh had been brought to that hospital after the second battle of Bull Run, and after the battle of Antietam. The patients had remained long on the battle-field without nourishment. They had exhausting suppuration, and, with a solitary exception, they all died.

The want of good nursing is also a very serious evil, and greatly diminishes the chances of recovery. Another evil of serious importance and of very frequent occurrence in armies, is the want of proper skill and experience on the part of the attending surgeon.

From one or more of these causes, gun-shot fractures of the thigh often prove destructive to life, in some cases very speedily after the infliction of the injury, and in other cases at a more protracted period. In cases in which the life of the patient is not sacrificed his health is often undermined in consequence of long continued suppuration, of privation of active exercise, and of other depressing agencies which occur in connection with the injury. And even where the fractured bone has become reunited, and the accompanying wound has perfectly healed, and the patient has regained a good degree of health, the limb is often so deformed, being shortened, bent, or twisted, that it is comparatively useless to the patient as an organ of support or locomotion. Indeed, the limb is often deformed to such a degree as to be an absolute incumbrance to the patient, and he looks forward with satisfaction to the relief which can only be afforded by the amputation of the offending member. Independently of the shortening, bending or twisting of the bone, the muscles often become so wasted and agglutinated together by the destruction of the cellular tissue, in consequence of long continued suppuration, that the limb is of little or any service to the patient. But, on the other hand, there are some cases in which the life of the patient is preserved, the constitutional vigor is unimpaired, and the limb is sound and strong and capable of performing its functions in a satisfactory manner.

In every case of gun-shot fracture of the thigh, the first question which presents itself to the surgeon who has charge of the patient is, whether an attempt shall be made to save the limb, or whether it shall be amputated. There are several important circumstances to be taken into consideration in answering this question. In the first place, is the injury of such a nature as to expose the life of the patient to very imminent danger, rendering his recovery almost hopeless without removal

of the limb? In the second place, would the amputation of the limb afford a reasonable prospect of saving the life of the patient? In the third place, would the limb, if saved, be of sufficient utility to the patient to justify the exposure of his life to any great additional hazard in the attempt to preserve it? Each of these three questions may be answered affirmatively in some cases, and negatively in others. In each individual case, the surgeon should aim at a correct solution of each of these questions, under the circumstances in which he is placed. I will endeavor to lay down certain rules, which, in my opinion, will be useful in solving the question of amputation.

I. When an army is engaged in active military operations in an enemy's country, and at a great distance from its base, or when it has cut loose altogether from its base, as in Sherman's great march across Georgia and the Carolinas, almost every case of gun-shot fractured thigh should be submitted to amputation, as the transportation of the patient with his fractured limb would be an occasion of extreme and protracted suffering, and would deprive him almost entirely of any chance of recovery which he might otherwise enjoy. Of course, numerous exceptions to this rule may occur, where the inhabitants of the country are humanely disposed, and where the patient may safely be placed under their charge. There is no great danger in transporting a patient for many successive days after amputation of his thigh, if the stump be properly supported and the transportation be not too rudely effected. Indeed, patients often do better under these circumstances than when they are crowded together in large hospitals, where they enjoy entire rest, but where, at the same time, they breathe a more or less infected atmosphere. I conversed on this subject with Dr. Thomas J. Watson, surgeon of the 32d Missouri Volunteers, who had been nearly four years in the service, and who was attached to Sherman's army in its long march through Georgia and the Carolinas. During this march, amputation was performed in nearly all cases of gun-shot fractures, both of the upper and lower extremities. The amputated cases did better during transportation than in hospitals. Near Macon, seven primary amputations were performed for gun-shot fractures, and six of them recovered.

II. Amputation should be performed in nearly all cases of gun-shot fracture of the thigh in which the femoral or popliteal vessels are wounded. When the injury is so high up as to admit of amputation only at the hip-joint, there may be a question as to the expediency of the operation, as primary amputation at the hip-joint is almost inevitably fatal.

III. Amputation should be performed in all cases of gun-shot fracture of the thigh in which the ball has passed fairly through or into the knee-joint, or in which the bone has been comminuted at its inferior articular extremity. When the joint has been but slightly exposed on one of its lateral surfaces, and the fracture involves only the superficial part of the external or internal condyle, an attempt may be made to save the limb, although, even under these circumstances, the issue of such an attempt is extremely doubtful.

IV. Amputation should be performed in all cases of gun-shot fracture of the thigh below the junction of its middle and upper third, when the bone is extremely comminuted over a space of four inches or more of its length. When a comminuted fracture is very near the upper extremity of the bone, there is reason for more hesitation as to the amputation of the limb, as the operation at the hip-joint, or in its immediate vicinity, is followed by a speedily fatal result in so large a proportion of cases.

V. Amputation should be performed in all cases in which the injury is inflicted by a cannon ball, or a large fragment of shell, and in which the soft parts are so disorganized that they must necessarily lose their vitality. If, however, the shock of the injury occasion extreme depression of the vital powers, from which the patient does not rally, it is better to let him die in peace than to hasten his death by the performance of an operation which he has not strength to endure.

When an attempt is made to save the fractured limb, great care should be taken to avoid all needless sources of irritation, to keep the limb in an extended position, and to give it such mechanical support as will prevent the soft parts from being penetrated by the sharp fragments of the bone. It is especially important that the limb should be well supported during transportation; and the preservation of the life or limb of the patient will depend on the attention which is paid to this circumstance.

At an early period after the injury the wound should be carefully examined by the introduction of a finger, and any loose fragments of bone which may be detected should be extracted; when it is necessary, the wound may be enlarged for this purpose. The fragments can usually be extracted more readily through the hole of exit than through the hole of entrance. When the ball has not passed entirely through the limb, but has approached the surface opposite to the hole of entrance, it may be advisable to make a counter-opening, both for the extraction of the ball and for the more ready removal of de-

tached fragments of bone. Such counter-opening will also contribute to the more effectual drainage of the wound. If there be sharp spiculae of bone, which have a strong tendency to protrude through the wound, they may be removed by means of a saw, or of a cutting or gnawing forceps. If there be any tense bands of aponeurosis stretching across the wound, it will be well to divide them with a bistoury, guided by the finger. When suppuration has become established, it is a matter of the greatest importance to secure a free and direct outlet for the pus, and to prevent it from burrowing among the tissues. It is also important to prevent the surface of the limb from being continually bathed in pus, and to guard the clothing of the patient, as well as the bed and bedding, from being soiled with the discharge. To accomplish these indications, free counter openings should be made wherever the matter approaches the surface, compresses should be laid over the spaces between the openings, and bandages should be applied with moderate firmness, to press the matter towards the openings by which it is to be discharged. Opposite to these openings holes may be cut in the bandages, and lint, flax, or oakum may be inserted beneath the margin of these holes to prevent the discharge from soiling the bandages. The bed and bedding may be protected by means of oiled silk or India-rubber cloth. All these precautions were taken with excellent effect by Dr. Geo. K. Smith, in the Armory Square Hospital, at Washington.

The interior of the suppurating cavity may be thoroughly cleansed, once or oftener in the day, by a copious injection of soap and water, or other detergent liquid. During the period of suppuration the strength of the patient is to be supported by a generous diet, and tonics and stimulants are often required for the same purpose. The safety of the patient, and the usefulness of his limb, will depend very much on the care and attention with which these rules are carried out in practice. The importance of avoiding any mechanical disturbance of the fractured limb during transportation is illustrated in the case of Lieut. Lowry, of the 146th N. Y. Volunteers, whose case is the second in my collection. He was a young man of sanguine temperament, good constitution, and regular habits, whom I saw in camp on the 22d May, 1865. He had been wounded at the battle of Cold Harbor, on the 3d June, 1864. The ball had entered his right thigh, on its inner side, a little above its middle, and passed obliquely upward, outward and backward, presenting itself under the skin behind the great trochanter, where an incision had been made and the ball extracted. The femur had been broken in its upper fourth, and a very

unfavorable prognosis had been pronounced at the time. He had, however, been placed upon a stretcher, upon which his limb had been properly supported, and upon this he had been carried by hand to White House, and thence, on the same stretcher, to a steamer, and on his arrival at Washington he had been carried, upon the same stretcher, to his ward in Armory Square Hospital. In the hospital he was treated by placing his limb in the fracture-box, which was in ordinary use in that institution, without extension. His recovery had been rapid, and at the time of my observation he was in good health. His limb was shortened two inches, and he walked with a cane, which he hoped soon to be able to lay aside.

If he had been changed from stretcher to ambulance, from ambulance to steamboat, from steamboat again to stretcher, from stretcher to ambulance, from ambulance to stretcher, and thence to his bed in the hospital, as was too often the case with patients having compound fractures of the thigh, instead of a happy recovery with a sound and useful limb, he might have occupied a Southern grave, or have had an imperfect recovery, with a shattered constitution, and a deformed, emaciated and useless limb.

Among the gun-shot fractures of the thigh which I saw in the hospitals of Washington and Baltimore, I found that a number of different methods of treatment had been resorted to. The method which impressed me most favorably was that which is known as Dr. Buck's method. It consists essentially of extension, made by a weight attached to a cord, passing over a pulley, and secured to a block of wood below the sole of the foot, the middle of a long strip of adhesive plaster passing across the block of wood, and the ends being applied to the sides of the leg and thigh, and kept in place by spiral strips of adhesive plaster and a roller bandage. By this means the pressure of the extending force is equalized upon the sides of the limb, and no injurious pressure is made upon the instep. A brick being placed under each foot-post of the bed, the weight of the body generally secures a sufficient amount of counter-extension. I had long been familiar with the excellent results of this method of treatment in simple fractures of the thigh, as they occur in civil practice, whether in adults or in children. This method has been found advantageous in simple fractures, under my observation, in promoting the comfort of the patient during the treatment, and in securing union of the fractured bone with the least possible amount of shortening or other deformity. From the experience which I had had in the treatment of simple fractures of the thigh by this method, I was, in some

measure, prepared to appreciate the results of the same method as applied to the treatment of gun-shot fractures. And I was very much gratified with the opportunity which I enjoyed of observing the satisfactory results of this mode of treatment, especially in the practice of Dr. Geo. K. Smith, in the Armory Square Hospital. Doctor Smith is a graduate of the University of New York, and has availed himself of the opportunities which he enjoyed of seeing the surgical practice of the hospitals of this city, and of familiarizing himself with the details in the management of fractures; and having good powers of observation, and a large amount of mechanical ingenuity, he has devoted himself to the treatment of fractures with a degree of industry and skill not surpassed by that of any of the other surgeons whom I met in the military hospitals which I visited. Since Dr. Smith has been in charge of the principal fracture wards of the Armory Square Hospital, Dr. Buck's method of treatment has been adopted in almost every case; and the testimony of Dr. Smith, and of the other surgeons who are associated with him, is decidedly in its favor. In most cases the patients are more comfortable with than without the extension; in a few instances it is a matter of comparative indifference to them, as far as their comfort is concerned. In only one instance did the extension occasion positive distress, so as to compel its discontinuance for a time. In the case alluded to an abscess was forming in the popliteal space when the patient was admitted to the hospital, and the extension, by the weight and pulley, gave rise to increased pain; but when the abscess had become mature, and the matter had been discharged, the weights were reapplied, and the patient was made more comfortable by their use.

The testimony in favor of this method of extension was not as strong in some of the other hospitals as in Armory Square Hospital, the statement being made that a number of the patients could not bear the treatment. My own impression, in these cases, was that there was a want of accurate knowledge as to the details of the treatment, and a want of skill in carrying it out. In some of the hospitals, this plan of treatment was entirely ignored.

In the Armory Square Hospital, the fractured limb is placed on Hodgen's skeleton splint, which does not interfere with the extension, while it facilitates the dressing of the wounds and sores. Some of the patients whom I saw in the hospitals of Baltimore and Washington had been treated by suspending the limb by means of Smith's anterior splint. In some of these cases very satisfactory results had been obtained.

I will now present a summary of the cases which came under my observation, arranged in groups, embracing the more important results of treatment. The first group embraces those in which there was no shortening of the limb—viz, cases 9, 11, 13, and 30. In cases 9, 11, and 13, the two limbs were of equal length. In case 30, the limb which had been fractured was nearly a quarter of an inch longer than the other. All these cases were treated by extension in the Armory Square Hospital, since the beginning of the year 1865, when Geo. K. Smith introduced the present improved method of treatment.

The second group embraces those in which the shortening did not exceed one inch, viz., cases 3, 16, 24, 25, 28, 29 and 41. The shortening in these cases was $\frac{7}{8}$, $\frac{3}{4}$, $\frac{5}{8}$, $\frac{7}{8}$, less than one inch, a little over $\frac{1}{2}$ inch, and one inch. Nos. 3, 16, 24, 25, 28 and 29 were treated by extension in Armory Square Hospital since the beginning of 1865. Case 41 was treated first at Potomac Creek Hospital, and then at Finley Hospital, in both which institutions the limb was suspended by means of Smith's anterior splint.

The third group embraces the cases in which the shortening was over one inch, but did not exceed two inches, viz., Nos. 2, 8, 14, 17, 19, 20, 26, 31, 33 and 45. Cases Nos. 8, 14, 17, 19, 20, 26 and 31 were treated by extension in the Armory Square Hospital since the beginning of 1865. Case No. 2 was treated without extension in the Armory Square Hospital at an earlier period. Case 33 was treated without extension in Judiciary Square Hospital. This case was remarkable for the fact that there was so little displacement that neither patient nor attending surgeon was aware of the existence of the fracture until the patient had been in the hospital a number of days. Case 45 was treated at Annapolis by means of Smith's anterior splint for four weeks, and then by extension, with a weight of twenty-five pounds, for fourteen weeks.

The fourth group embraces the cases in which the shortening was over two inches, but did not exceed three inches, viz., Cases Nos. 6, 12, 15, 18, 21, 22, 32, 34, 38 and 46. Nos. 6, 12, 15, 18, 21 and 32 were treated by extension in the Armory Square Hospital since the beginning of the year 1865. No. 22 was treated at City Point without extension. No. 34 was treated at Stanton Hospital without extension. No. 38 was treated at Carver Hospital without continued extension. No. 46 was treated at Camden Street Hospital, in Baltimore, for the first month by means of Smith's anterior splint, and for the second month by extension with one brick.

The fifth group embraces the cases in which the shortening was

more than three inches, but did not exceed four inches, viz., Nos. 5, 36, 40, 42, 44, 47 and 48. No. 5 had been treated at Armory Square Hospital from October 3d, 1864. No. 36 had been treated at Campbell Hospital without continued extension. No. 40 had been treated at Lincoln Hospital without extension. Nos. 42 and 48 had been prisoners in the hands of the rebels, and had had no treatment. No. 44 had been treated at Annapolis with Smith's anterior splint, without extension. No. 47 had been treated at Camden Street Hospital, in Baltimore, by Smith's anterior splint, without extension.

The sixth group embraces all the cases in which the shortening exceeded four inches, viz., Nos. 1, 4, 7, 10, 35 and 39. The shortening in these cases respectively was $4\frac{5}{8}$, 5, $4\frac{1}{2}$, $6\frac{3}{4}$, 5 and $4\frac{1}{8}$. No. 1 had been treated at Lincoln Hospital, without extension. Nos. 4 and 7 had been treated at Armory Square Hospital, without extension. No. 10 had been treated at Hilton Head by means of Smith's anterior splint, Hodgen's splint and a long extension splint. No. 35 had been treated at Campbell Hospital, without extension. No. 39 had been treated at Carver Hospital, without extension.

In reviewing the cases which have been presented, it appears that twenty-five of the whole number have been treated by extension at the Armory Square Hospital, since the beginning of the year 1865. In two of these cases the limbs were not measured, in consequence of the feeble state of the health of the patients at the time of my visit. In the remaining twenty-three cases, there were four in which there was no shortening; there were five others in which the shortening was less than one inch; there were eight in which the shortening was more than one inch, but did not exceed two inches; and there were six in which the shortening was more than two inches, but did not exceed three inches. The extent of the shortening did not exceed three inches in any of the twenty-three cases, and it only reached that amount in a single case. The average shortening in the twenty-three cases was less than an inch and a third.

There were nineteen cases treated without any methodical extension. The minimum shortening in a solitary case (No. 41) was one inch. This case had been treated with Smith's anterior splint. The shortening in another case (No. 33) had been only an inch and a quarter; but in this case it is remarkable that there was so little displacement from the beginning that the fracture was not detected until several days after the infliction of the injury. In No. 2 the shortening was two inches; in No. 7, two inches and a quarter; in Nos. 22 and 38, each two inches and a half; in No. 5, three and a quarter; in No. 47,

three and three-quarters; in Nos. 36, 40 and 44, each four inches; in No. 39, four and an eighth; in No. 7, four and a half; in No. 1, four and five-eighths; in Nos. 4 and 35, each five inches; in No. 10, six inches and three-quarters.

In two of the nineteen cases which were treated without methodical extension, there was no measurement. Of the remaining seventeen, there were but two cases in which the shortening was less than two inches. There were eleven cases in which the shortening exceeded three inches, and of this number there were nine in which the shortening was four inches or more. The average shortening in the seventeen cases was more than 3.55 inches. Excluding the two most favorable cases, the average shortening was more than 3.88 inches. There were two of the cases in which the patients fell into the hands of the rebels, and in which there was no surgical treatment. In each of these cases, union occurred with a shortening of four inches.

Of forty-eight of the cases observed, the fracture involved the upper third of the thigh in twenty cases; the junction of the upper and middle third in five cases; the middle third in nineteen cases; the junction of the lower and middle third in two cases; and the lower third in one case. In one case, the precise locality of the fracture was not noted. It is a remarkable fact that so few of the cases involved the lower extremity of the thigh. It is very improbable that there should have been so small a proportion of original injuries in that situation. It would, therefore, appear probable that a larger proportion of cases of fracture near the lower part of the thigh had proved fatal, or had led to primary amputation of the limb, in consequence of the knee-joint being implicated. The knee-joint is more frequently involved in fractures near the lower end of the thigh than the hip-joint in fractures near the upper end.

Dr. TRIPLER.—Mr. President: In regard to the points made by my colleague, as the result of observations made in the hospitals, I have no particular objection to make; they are undoubtedly entirely legitimate conclusions from what he has himself observed. I don't know that there is any thing extremely novel in them. With regard to this destruction of the soft parts of which he speaks as requiring amputation, nobody can make any objection. With regard to amputation generally, as required by gun-shot fractures of the thigh, we were taught, before the Schleswig-Holstein war, that amputation was a necessary resort in cases of fracture of the thigh-bone and fracture of almost any bone; and it was not until after that war that we were taught the new principle of exsection in contradistinction to amputa-

tion. I know that in the Mexican war (I was not in the first battles, but was informed by my colleagues who were in the battles of Resaca and Palo Alto) attempts were made to save limbs in cases of gunshot fractures of the thigh, but almost invariably without success, and the few cases that did recover were attended with great suffering and shortening of the limb. My first attempt to save a limb in that way was after we got to the city of Mexico. In this instance the patient was carried to one of the houses in the city and recovered; but recovered with shortening, which I regard as almost a necessary result of all fractures of the thigh. It is the rule, and the preservation of the limb with its natural length the exception. From the time of the Schleswig-Holstein war we have come to look upon exsection as the rule to be pursued, and amputation as the exception. Men recover with shortening as a matter of course. If there is a comminuted fracture, we attempt to remove all those detached spiculæ of bone; and those spiculæ are placed under several different classes. Dupuytren had classified them as primary, secondary, and tertiary: primary, those which are perfectly detached; secondary, those which are still adherent by periosteum; tertiary, those which are mere splits in the bone, still adhering, not being detached from the periosteum. These, of course, are to be removed, as foreign bodies. We attempt to save the limb, and it does not depend exactly upon whether the man is going to be left at rest or whether he is to be transported over roads. This matter of transportation is one of no little importance. We have been trying to introduce vehicles so constructed that the patient will be as much at rest in transportation over rough roads as he would be passing up Broadway, or over one of the beautiful macadamized roads in Central Park. This is a subject which is unattainable. As long ago as 1836, during the war in Florida, an officer was wounded at the battle of the Wahoo Swamp—his thigh-bone fractured. He was placed in a baggage-wagon, nothing under him but tents and bed sacks; was transported over as rough a road as you ever saw through the Palmetto Swamps, over roads in which the palmetto roots were running, making deep ruts; he was transported down to the little town of Volusia, and I expected to find him dead when we got there, but he had made the journey with comparative comfort. There were no springs under those wagons; he was carried over that rough road on a rough jolting vehicle, and had made the journey with comparative comfort, and arrived under very favorable prospects of getting along well. He was going by boat up to Black Creek, two or three hundred miles farther up the river; but the steady tremulous

motion of that steamer was sufficient to torture him to death, and he did die. So much, then, for ease of transportation in respect to the comfort of the patient. I am persuaded that no system of springs that you can make, I don't care how you adjust them, will insure the comfort or safety of a wounded man, and that the less spring you have to your wagon the better.

In relation to the treatment of these fractures, I think amputation should very seldom be resorted to. I think so now. I did not in 1856. Exsection of the fractured extremities of the bone has been resorted to, not only in the thigh and in the humerus, but in all the long bones. I have myself seen very few cases where there has not been a false joint; and during the last two years a few cases have been reported by the surgeon of a Michigan regiment where he has resorted to exsection in the continuity of the femur; two cases in which he has exsected the fractured portions; the extremities were brought together, and the result was a solid reunion of this bone, and his patients recovered with no more shortening than my friend Prof. Post has described; 3.88 I think he mentioned as the shortening in certain cases, and in the cases that fell into the hands of the rebels. While Batewell exsected his cases he had shortening. He had a solid union, and his patients are now walking about. These things have been done once and can be done again. I observe that Dr. Howard has been reading a paper before the Royal Medico-Chirurgical Society in London, upon the subject of exsection in the continuity of bones, and bringing the bones together, not by the simple mechanical contrivance Batewell used, but in using a wire suture. Now, if the reports we are going to receive hereafter from the Surgeon-General's office tell us that such means have been resorted to, and that successfully, it will introduce altogether new principles into the department of surgery relating to the treatment of gun-shot fractures in the continuity of bones. If we can by exsection remove the fractured bones, bring the two ends together, fix them by any mechanical contrivance whatever, it will be certainly a great step in advance of the old method of resorting to amputation.

There are a great many things to be considered in regard to this adjustment in the fracture of the thigh. I suppose everybody is familiar with the observations of Mr. Vincent, the Senior Surgeon of St. Bartholomew's Hospital, London, in regard to the position of the thigh in the upper third. The upper fragment, as we all know, rises above the lower, and there is no sort of use of bringing the lower fragment up into any position to adjust itself to the upper; because if

the upper is allowed to rise to a certain point, the muscles are in the most favorable position for their action. If you bring that upper fragment down to a horizontal position, the patient lying upon his back, you bring the muscles into the most unfavorable position for their action. The moment you begin to raise them, that moment you begin to obtain greater power of contraction—the best position for the man being on his back. The lower you bring them down, the less power the muscles have; and if you bring the lower portion to adjust itself to the upper, you bring them to the most favorable position. I think no one will dispute the fact that where you bring these muscles in a position so that, associated, they have the greatest possible force, there you will have the greatest possible displacement; that if you bring down the muscles as far as they can be, there they will have their minimum force, and the lower part of the limb being placed in a position to adjust itself to the upper, will have the less disposition to displacement. Adjust the weight so that it will be exerted in the direction of the axis of the limb, by passing the cord over the foot of the bed by a pulley; bring every portion down as far as the anatomy of the parts will permit, so that the muscles will have the least possible force, and you will have much less difficulty in preserving the limb in its proper position than you will by resorting to any other method. This is the rule: Amputation where the soft parts are extensively destroyed; amputation in all cases where *artery and vein* are both involved; attempt to save the limb where you have nothing but fracture of the bone itself, properly adjusting the fractured extremities, removing Dupuytren's primary spicula of bone, with proper extension keeping the limb down in a horizontal position.

NEW YORK PATHOLOGICAL SOCIETY.

Stated Meeting, Sept. 13, 1865.

DR. AUSTIN FLINT, President, (pro tem.,) in the Chair.

BONY TUMOR OF THE FINGER—DR. A. C. POST.

Dr. Post presented a very hard bony tumor which he had removed from the sheath of the flexor tendon of the little finger of a woman fifty years of age. The patient ascribed the origin of the growth to some contusion which she had suffered twenty years before, ever since which time the tumor had slowly and steadily increased in size. At the time of its removal it measured an inch and a quarter in its longest diameter and an inch in its shortest. The removal was accom-

plished without difficulty, and the mass, which was of almost flinty hardness, when sawn open presented the appearance of cartilage densely ossified.

CASE OF CHOLERA INFANTUM—DR. LEWIS SMITH.

Dr. LEWIS SMITH exhibited the intestines taken from a child ten months old, who had been a patient in the Infants' Asylum. The little patient was brought up by hand, and had had looseness of the bowels nearly or quite all summer. Dr. Smith's attention was not particularly directed to it until about the first of the present month. The evacuations at this time numbered four or five daily—were sometimes green, sometimes dark, and quite offensive. It was moderately emaciated, and there was no vomiting at this time. It began to be very fretful, and, that fretfulness continuing, Dr. Smith was led to attribute it to the brain. There was no decided change in the child, excepting it was becoming somewhat weaker, until the 8th of September, when a state of drowsiness supervened. On the 9th the patient began to sink, and on the 10th died of exhaustion. There were no convulsions, and it was apparently conscious to within a few hours of its death.

On the following day the autopsy was made. On opening the skull the membranes of the brain presented their usual appearance, except that overlying the surface of the vertex and between the arachnoid there was some transparent serum, about an ounce or an ounce and a half in quantity. There was only a moderate increase in the vascularity of the organ, and there was no undue amount of serum in the ventricles. The thoracic organs were healthy, except the *œsophagus*, which was moderately inflamed. The stomach presented no unusual appearance whatever—an important fact, considering that it is generally believed that in the so-called cholera infantum the mucous membrane is softened. There was nothing abnormal in the intestinal canal until within a foot from the ileo-cæcal valve; here patches of thickened mucous membrane showed themselves, and at a distance of four or five inches from the valve the membrane was uniformly thickened and red, showing that it had been the seat of intense inflammation. The ascending colon was in nearly a normal state, while in the descending portion the solitary glands were quite distinct. On reaching the rectum the condition of things was somewhat similar to that of the ileum already described. The mesenteric glands were but slightly enlarged.

The points of interest in the specimens were, first, with reference to

the condition of the brain. The Dr. stated that during the past summer, and during previous seasons, he had examined quite a number of children under the ages of three and four months, during that period in which the cranial bones were not consolidated, and had uniformly found that serous effusion was not present. This absence of serum he explained, by supposing that the brain during the progress of the disease became shrunken, and that the cranial bones overlapped and adapted themselves to the shriveling organ, thus rendering the effusion of fluid unnecessary, there being no such vacuum formed as in those cases in which the cranial bones were consolidated and unyielding. The second point of interest had reference to the unusual situation of the intestinal lesion in such cases, it being for the most part confined to the lower portion of the ileum.

In answer to a question from Dr. Newman, he stated that he had never used the nitrate of silver in the treatment of the disease.

Dr. NEWMAN then stated that during the past summer he had found that the administration of the nitrate of silver, in doses of $\frac{1}{12}$ th of a grain for a child a year old, every three or four hours, was attended with marked benefit. If there was much restlessness, he was in the habit of adding a little paregoric.

In reply to a question from Dr. Post, Dr. Smith remarked that in cholera infantum there is generally vomiting present, only, however, within a day or two of death.

Dr. POST stated that he had always looked upon vomiting as a very important symptom in the disease described as cholera infantum, and from that fact was disposed to think that Dr. Smith's case could not properly be styled any thing more than diarrhœa.

Dr. SMITH remarked that the disease of which the child died was, as he understood it, generally known with the profession in accordance with the appellation which he had given.

Dr. ELLSWORTH ELIOT agreed with Dr. Post in reference to the proper name for the disease, and stated that he looked upon cholera infantum as a malady which had a very rapid progress, and was attended with watery discharges and severe vomiting. Dr. Eliot asked what was the highest point in the intestine at which Dr. Smith in his post-mortem examinations had found the green color to the contents of the intestine, and also referred to what he believed was the generally received opinion in the premises that they were due to the altered bile.

Dr. SMITH did not think that he could mention the highest point at which he had found the green color referred to, but would state that he had seen yellow matter in the jejunum and green matter lower

down, a fact which would seem to prove that the bile had little if any thing to do with the production of the effect. He concluded that the green color was due to some alteration which was suffered by the intestinal mucus.

Dr. Post suggested that it might be due to the bile, notwithstanding, inasmuch as that secretion might naturally be changed in character as it passed down the intestine.

ANEURISM OF THE INNOMINATA—DR. AUSTIN FLINT.

Dr. FLINT exhibited an aneurism of the innominata, which presented rather a special interest in reference to a point in diagnosis. The trachea was flattened and pushed to one side, but the relative position of the recurrent nerve, on account of the premature drying which the specimen suffered, could not be ascertained. Inasmuch as Dr. Day, of Bellevue Hospital, intended to publish the history of the case in full, he did not think it worth while to give any minute account of the patient's condition, except so far as it related to the points in the diagnosis. This patient, continued he, entered Bellevue Hospital, and was suffering from considerable embarrassment in respiration, requiring to be much of the time in the sitting posture. One interesting point in connection with the posture was, that the patient experienced most comfort in inclining towards the left side, the reverse of what would be expected from the position of the tumor, pressing as it did so decidedly upon the trachea. The patient had some huskiness of the voice, which, however, was explained by the existence of a slight laryngitis. There was also a peculiar sound in the respiration, apparently referable to the larynx, which I can not well describe, but which, having observed in certain cases of aneurism involving the recurrent nerve, I have learned to regard as distinctive. I may say here that from this symptom I was enabled to suspect the existence of an aneurismal tumor, even before a physical examination was made. This sound, I presume, is occasioned by the paralysis of the muscles of one side of the glottis.

On proceeding to examine this patient none of the physical signs of aneurism were discovered; we could not even make out distinctly dullness on one side, which might be considered surprising, considering the size of the tumor, which can only be explained by its being confined so nearly to the median line. There was no bellows murmur appreciated, notwithstanding repeated examinations were made. There was a disparity in the pulse, not very marked, but constant and distinct; the radial pulse on the right side was markedly less in force

than the pulse of the left wrist. There was also a disparity in the size of the pupil, the left being more dilated than the right. There was no pulsation or thrill.

I present the specimen to illustrate the value of the points which led more particularly to the formation of a diagnosis, viz.: the peculiarity in the respiration, and the disparity in the pulse and in the pupils. In connection with this case it is interesting to note that a patient is now an inmate of Bellevue who presents a distinct disparity in the radial pulse, habitual, labored breathing, and in that instance the physical signs of aneurism are also very indistinct; there is, however, distinct dullness on the right side of the sternum, but no bruit, and the heart-sounds are very much more distinctly heard on the right side of the sternum, at the base of the organ, than at any other point in the præcordia.

In answer to a question from Dr. Messinger, he stated it as his impression that the spasmodic cough was not a very common accompaniment to aneurism.

LARYNGEAL SYMPTOMS IN ANEURISM OF THE INNOMINATA—DR. A. C. POST.

Dr. Post referred to the following case, which had presented itself to him two years ago:

A patient complained at first of some irritation of the throat, which was thought to be owing to an elongated uvula. This organ was accordingly removed. The huskiness of the voice which also previously existed, however, remained, and the Dr. lost sight of him until two or three months ago, when he again presented himself, and it was found that an aneurism, supposed to be connected with the innominate, existed in the situation of the sterno-clavicular articulation, causing a marked protuberance at that point. Dr. Post supposed that the aneurism had been forming from the time that the huskiness of the voice first showed itself, and that this symptom was the only one at the time which pointed to the existence of the trouble. He further remarked that these laryngeal symptoms connected with aneurismal tumors were often mistaken for those which were due to laryngeal disease, and that tracheotomy had often been performed for their relief. He believed that this operation was often attended with benefit, even when the mistake in diagnosis was made.

Dr. FLINT concurred in the opinion expressed by Dr. Post in reference to tracheotomy, and alluded to a case which he had published in the *Medical Times*, in which a patient was evidently destroyed by spasm of the glottis, induced by a small aneurism, an event which might have been prevented had the operation of opening the trachea been performed.

Stated Meeting, Sept. 27, 1865.

Dr. GURDON BUCK, President, in the Chair.

WOUND OF THE POPLITEAL ARTERY—DR. L. A. SAYRE.

Dr. SAYRE exhibited a specimen of wound of the popliteal artery, accompanied with the following history, drawn up by Dr. W. A. Lockwood, House Surgeon, Bellevue Hospital.

John Graham, a native of Ireland, aged 26, was admitted to Bellevue Hospital, Friday morning, June 20th, 1865, at 3 o'clock, and gave the following statement in regard to himself, (which is not to be taken as truth.) According to his own account he became intoxicated on Monday evening, June 19th, and when in that condition in his house, at 11 o'clock, he fell from a chair, striking upon a large knife so placed as to enter his left leg. A gush of blood followed, and a surgeon was called who plugged the wound with pieces of cloth, and also compressed the parts firmly at the point of injury by bandaging. He was then sent to the hospital, and I saw him at about 3 o'clock, Tuesday morning. He was laying in bed and looked as persons do after a severe hemorrhage. The bleeding seemed nearly arrested at the time by the compression which had been made by the surgeon outside. I arranged a tourniquet on the femoral artery in Scarpa's space, and also a compress lower down, and removed the compression from the injured parts. This was followed by a gush of bright blood, which was instantly arrested by screwing down the tourniquet above. I then proceeded to examine the wound, and found it to be a cleanly cut opening, about one inch in length, on the outer aspect of the left leg, just above the knee-joint, and anterior to the tendon of the outer hamstring muscle. It penetrated the tissues of the leg, and seemed to go in a direction behind the thigh-bone, and just above its condyles. Being convinced that the popliteal artery was wounded, I continued to control the femoral with compress and tourniquet, and having applied picked lint firmly bandaged over the wound, the limb was bandaged from the toes upwards, and elevated upon an inclined plane.

Dr. Lewis A. Sayre arrived at 11.40, A.M., and first tied the circumflex artery in the wound, which he found filled with clotted blood, and with the pieces of rags which had been used to plug the wound. He then made an incision about five inches in length in the popliteal space, and having brought the popliteal artery well into view, found that a cut about one eighth or more nearly one quarter of an inch long had been made through the wall of the vessel in the direction of

its length, without dividing it. This cut gaped open and a large flat stream of blood was poured out, which was arrested by digital pressure, made upon the wounded vessel. On everting the edges of the cut, the inner serous coat of the artery could be plainly seen. Dr. S. tied the vessel on each side of the wound, which was then left open at its central part to afford exit for the blood and discharges. The original wound, which was not dependent, was closed by strapping. Stimulus being indicated was freely given after the operation; but the man seemed to have lost quite an amount of blood before entering the hospital, and did not rally well at any time. The day after the operation, (Wednesday, June 21st,) the wound was dressed with oakum soaked in diluted Labarraque's solution. There was some bad odor present, and considerable blood had been infiltrated into the tissues before the artery was tied. The patient's face looked white, and his pulse was not strong. Stimulus continued. At 4 o'clock in the afternoon of this day the man became somewhat delirious, and attempted to get up. His leg was examined and dressed at 6.30, P.M., and did not look markedly worse, and he appeared to sleep somewhat during the early part of the night. At 4 A.M., Thursday, it was very evident that the man was growing weaker and more delirious, and when his leg was uncovered to be dressed, about two hours afterwards, it was found looking dead and black, and was very much swollen.

There had been no actual sloughing, but the whole limb seemed dead. The man died at 6.40, A.M., Thursday, June 22d. After death Dr. Sayre took out a portion of the popliteal artery, in which the wound could be plainly seen, and the ligatures were found well placed on each side of it.

ANEURISM OF THE AORTA.—DR. A. JACOBI.

Dr. JACOBI presented an aneurism of the arch of the aorta. The history of the case was, from the attendant circumstances, necessarily very incomplete. The gentleman who removed the specimen only saw the patient twenty-four hours previous to death. A man aged 25 years, thin and emaciated, was found by him in great dyspnoea, perspiring, with a pulse of 85 and a feeble heart's action. There were, however, no sounds developed on auscultation which gave the physician any suspicion of the existence of an aneurism, but he found a dull percussion sound on the left side corresponding with the position of the lower lobe of the left lung. The following day, when a visit was made, the patient was in the same condition, until sometime in the evening, when his dyspnoea increased all at once, and shortly after, uttering a

sudden cry, he was dead. A post mortem examination was made when the cause of death was found to be the rupture of an aneurism of the aorta. The rent was large, part of it being covered by adherent lung.

It appeared that hemorrhage had taken place at two different periods. Probably the first one occurred when the physician was first called to the patient, but this rent was not only not large enough to allow much blood to enter the pleural cavity, but the lung adhering to the sac prevented much blood from oozing out. Then, it appears, at the time when the patient uttered his sudden cry, that a complete and final rupture took place, filling the left pleural cavity to that extent as to completely compress the lower lobe of the left lung, and part of the lobe of the upper. The aneurism was of the sacculated variety.

ARREST OF DEVELOPMENT IN THE KIDNEYS AND OCCLUSION OF THE URETERS
AND DUODENUM.—DR. JACOBI.

Dr. JACOBI also presented a second specimen which was removed from an infant who died when four days old, and to whom he was called a few hours subsequent to birth for the purpose of being consulted as to the removal of a congenital anomaly about the elbow joint; the thumb of the left hand was wanting, and there was an absence of the flexors of the forearm. I saw the child, said he, and went away with the intention of calling again, inasmuch as my attention was directed to the fact that it had not yet passed any meconium. I at that time introduced my finger into the anus, and also a probe, and after a good deal of effort succeeded in removing some of the contents of the intestine; I then left, feeling quite sure that some more meconium would follow. Until that time, seven or eight hours after birth, no water had been passed, but I was not then made aware of the fact. I did not see it again until three days afterwards, when the father came for a certificate of death. From his description of the case since I saw it, viz., that it had not passed any water, neither more meconium, and that it had vomited for the last two days, I concluded that death had resulted from some congenital anomaly, probably from some occlusion of the intestinal tract somewhere above. I accordingly declined giving a certificate until a post-mortem should be obtained.

I opened the infant and found, first, a rather small bladder. The parents and nurse knew positively that the child had not passed water during life. The bladder as seen here is well formed, but has only the

size of that organ as seen in a child six months old. The two ureters could be filled up to a certain extent by means of inflation with a small probe-sized canula, but when a short distance from the kidney were entirely lost in the surrounding connective tissue. The kidneys were both very small, and seemed to be made up principally of a pelvis, while the other more solid substance gave no marked distinction between a cortical and tubular portion. They were evidently, from their size and structure, unfitted for the secretion of urine. The stomach was much dilated, as was also the upper portion of the duodenum. Below this dilated portion of the duodenum, there was an abrupt construction, beyond which point the intestinal tract could not be inflated from below. At times, as in this instance, I have succeeded in inflating the gall-bladder through the ductus communis, showing that the obstruction must exist above that point where this duct empties into the intestine.

I am inclined, from the length of the intestinal tract below the obstruction, and from the condition in which the bladder is, to think that the inflammatory process which was the cause of both the obstruction of the intestine and ureters as well as the apparent atrophy of the kidneys, took place at about the same time, say the sixth or seventh month of pregnancy. At all events, the bladder is about the size of a six months' foetus, and the fact that there was well formed meconium below the obstructed portion also goes to prove that the intestinal tract performed its full function until the obstruction took place. That this is only the result of an inflammatory process is certain, and I am also convinced that the arrest of development in the kidneys and occlusion of the ureters can in a like manner be explained.

The child had been carried to full term, but was rather puny. I should not have taken it for more than an eight months' child, and do not suppose that it could have weighed more than five and a half pounds.

PHOSPHORUS NECROSIS—DR. J. J. HULL.

Dr. HULL presented a specimen of two halves of the jaw removed for phosphoric disease, together with three photographic views of the patient before the operation. The following is the history of the case and operation.

Catharine K., aged 22, a native of Germany, and unmarried, entered St. Luke's Hospital, September 6th, 1865, with the following history: She had been in this country eighteen years, was of a healthy family, and had always been well herself until the trouble for which she came

to the hospital commenced. Two years ago, after having been employed in a lucifer match factory about five years, she had three carious teeth (two molars of the right and one bicuspid of the left side) extracted, remaining at home two weeks afterwards, when she resumed work in the packing-room, in which department she was employed during her entire stay at the factory. At the end of nine months (fifteen months since) she, for the first time, experienced pain in the right side of the lower jaw; she then went to a dentist, who removed the remaining molar, telling her that the jaw had been injured by the barber who had extracted the other teeth. From this time she remained at home, never going to the factory again, recognizing her trouble as one arising from the business in which she had been engaged, being familiar with the disease from seeing it in others working at this trade.

On admission, she presented a marked cachectic appearance. The face was very much disfigured by extensive swelling, more marked on the left side. An exceedingly offensive odor came from the mouth, from which saliva, mixed with pus, constantly dribbled. Diseased bone could be felt through two fistulous openings on the right side of the neck, and one upon the left. The periosteum was much thickened, and easily detached from the jaw on both sides as far as it could be reached—this being impracticable to any great distance, owing to the limited extent to which the two jaws could be separated. The teeth were very loose, and some of them the patient removed and replaced at pleasure. Much pain was caused by any examination of the diseased parts. She was placed on a nourishing diet and tonics, and a mouth-wash of a weak solution of permanganate of potash ordered.

On Wednesday, September 13th, 1865, I proceeded to remove the entire lower jaw without external incision, being induced to operate in this manner in consequence of the success attending similar cases in the practice of Drs. Wm. Hunt and C. S. Boker, of Philadelphia, (*vide* American Journal of the Medical Sciences, April, 1865,) and of Dr. Weir, at St. Luke's Hospital, in this city, on the 2d of last August. In Dr. Boker's case, the entire lower jaw was removed; in the other cases rather more than one-half only. Dr. Weir, however, proposes to remove the remainder in his case at an early date. These, as far as I can ascertain, are the only instances in which the lower jaw has been removed without external incisions. The patient being etherized, and the tongue secured by a strong ligature, and this held by an assistant, the jaw was divided at its symphysis by the chain-saw, this being introduced by means of its needle through a puncture just below the point

of the chin, and carried behind the diseased bone. The mouth was forced widely open by Gross's lever and a cheek-holder, and by means of a cranial elevator and a curved blunt instrument about the width and thickness of an ordinary scalpel at its middle; the right side of the jaw was stripped of its periosteum over nearly its entire extent, within and without—this process having been in part accomplished, with a view to facilitate the operation, a few days previously. The ascending ramus was then seized with a powerful bone forceps, and by a series of twisting movements the whole of the right half of the bone was removed. The left half was then subjected to the same treatment, but this was found to be much more difficult of removal, owing to the firm attachment of the tendon of the temporal muscle to the coronoid process, and that of the external pterygoid to the condyle, the disease apparently not having extended quite to the articulations on this side. After a number of vigorous efforts made to remove this half, like the other, with the forceps, the tendon of the temporal muscle was finally divided with a blunt pointed straight knife, carefully guided by the forefinger of the left hand; it was then removed with the forceps, the pterygoid muscle having been ruptured by the previous traction exerted, as is shown by the specimen. During the latter half of the operation, a small vessel was wounded, which bled freely for a while, but soon ceased spontaneously. An examination of the removed jaw shows it to be necrosed throughout, except at the articulations on the left side. It should be remarked that, in performing this operation, it was originally intended to leave only the periosteum behind; but in attempting to detach it from the necrosed bone, the end of the instrument used got between this and the involucrum, the latter being very adherent to the periosteum. This having taken place to some extent before being discovered, it was decided that it would be better to proceed with the operation in this way, and leave the involucrum as a support for the soft parts, as well as to see to what extent, if any, it would assist in the formation of the new bone. It was accordingly left entire on the right side, and on the left as far as the angle—it, beyond this point, adhering firmly to the bone, as may be seen by the specimen. Thus, the position of the face was not much altered by the operation. On the following day considerably increased swelling of the face, particularly on the left side, was observed, with some febrile action, but nothing untoward took place until forty-eight hours after the operation. Then a slight friction sound was heard over the lower portion of the left lung, anteriorly, attended with the usual constitutional symptoms. A large fly-blister was at once applied, and by the next day the patient

was much better, being able to fill her lungs more freely, the friction sound having disappeared. On the following day, however, she became much worse, and on the 19th died, very extensive effusion having taken place in the left pleural cavity.

I regret that a post-mortem examination could not be had, though every effort was made to obtain permission from the friends.

Dr. BUCK remarked that the pumice-stone excrescence could hardly be considered as made up of true bone. Generally, this excrescence was very adherent to the bone, and came away with it; in Dr. Hull's case, however, there was an exception to this rule.

Dr. KRACKOWIZER believed that this pumice-stone deposit was, in the first place, well formed bone, which had afterwards become necrosed, and suffered a granular degeneration. This degeneration of the involucrum, he thought, was quite peculiar to the disease, and might be explained, in part, by presupposing the existence of a peculiar action in the bone before the necrosis took place, and partly to the condition of the periosteum on the bone. At all events, it was clearly shown that such deposits, which remained adherent to the periosteum, were capable of being transformed into healthy bone.

DOUBLE COMPLICATED HAIR-LIP—DR. GURDON BUCK.

Dr. BUCK exhibited a specimen which was one of abnormal development, and consisted of a cast taken from the face of a girl 10 years old, who, though otherwise well constituted, had the misfortune to have a double complicated hair-lip. The complication consisted in the presence of a rather remarkably prominent intermaxillary bone, supporting three incisor teeth, the cleft passing entirely through the uvula as well as the velum.

An operation was performed, which consisted in dissecting up the flap attached to the nose, the median central flap. That was detached then as the first step, and held in reserve. The projecting intermaxillary bone was then pared off on a horizontal line. That flap was then trimmed, and the edges squared, so as to cover the inferior edge of the fresh cut septum. The lip on either side was very freely detached as far outward as the molar teeth, so as to facilitate the advancing forward of the two sides, and bring them into approximation. The edges were then trimmed, and secured by twisted and interrupted sutures. The sequel of the case was in every way favorable. Within a week all the sutures were removed, and a photographic view shown proved how complete the result of the operation had been.

Dr. SAYRE referred to a case of deformity very similar to the one

presented by Dr. Buck, and stated that in that instance he had left the septum, crowding it back somewhat, to form a support for the base of the nose. The nose, under these circumstances, would be turned up a little, but the advantages gained in the support more than counter-balanced it. This case was operated upon ten years ago, the third day after birth, and the result showed the wisdom of the decision to interfere thus early. Although two incisor teeth were removed at five years of age, the bone was firmly adherent, and at ten years of age every vestige of the operation, as far as the bone was concerned, had disappeared. He advocated the practicability and propriety of performing all such operations as soon after birth as possible; the child for the first two or three days would sleep a great deal, was very quiet, and the healing of the soft parts was generally so rapid that the mouth would be in a condition to perform the function of nursing when the mother's breast should be ready. He was informed by Dr. Jacobi that authorities agreed in recommending the practice of early operating, and was delighted to know that his experience of ten years ago was finally substantiated and recommended.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Principles of Surgery. By JAMES SYME, F.R.S.E., Surgeon in Ordinary to the Queen in Scotland, Professor of Clinical Surgery in the University of Edinburgh, etc., etc., etc. To which are appended his Treatises on "The Diseases of the Rectum," "Stricture of the Urethra and Fistula in Perineo," "The Excision of Diseased Joints," and numerous additional contributions to the Pathology and Practice of Surgery. Edited by his former pupil, Donald Maclean, M.D., L.R.C.S.E., Professor of the Institutes of Medicine, and Lecturer on Clinical Surgery, Queen's University, Canada. Philadelphia: J. B. Lippincott & Co., 1866. 8vo., pp. 880.

This work, says the author, in his preface, "was the text-book of my systematic course, and contains the principles which I have endeavored to illustrate in my clinical lectures during the last thirty-four years. In its composition my aim has not been to collect all that might be said in regard to each subject, but rather to select what seemed of most importance, and arrange it in a convenient order for teaching or

study, so as to constitute a framework of surgical science, which might be filled up through the gradual acquisition of professional knowledge." Of course reference is had in these remarks to the general portion of the work only—the appendix, comprising 349 pages, embracing various monographs and other surgical papers.

Mr. Syme's reputation and influence, it need hardly be said, are very great; and a book from his pen can not but command respect. He has diligently used large opportunities; he is a man of decided opinions, and knows how to express them forcibly. In looking over the pages of the volume before us, we are struck with the terseness and energy of the language, and the practical character of the matter conveyed. One can easily imagine the charm as well as the value of the oral teachings of a man who writes thus.

We have here an embodiment of the views and experience of one of the most eminent and successful teachers and practitioners of surgery in Great Britain. Like many other writers of force and originality, Mr. Syme gives no extended quotations, or long list of references. He offers his own results, evidently with a firm conviction of their soundness; and we believe they may be consulted with profit by any one who will weigh them understandingly. Our reason for so speaking is that, in our opinion, so many practitioners err in following blindly the authorities. It is easier to adopt an idea than to test it; and the history of medicine is full of instances in which errors have been propagated, and the advance of science hindered, by too great a reverence for ancient landmarks. At the present day, perhaps, the tendency among our leading writers is rather toward skepticism; but the time will never come when there shall be no masses feeling the need of guidance. The surest road to the development, for these masses, of sound principles and successful practice, is through the free and intelligent canvassing and comparison of theories.

In glancing over the first portion of Mr. Syme's work, we notice that the pathological views set forth therein are not of the latest date. They are, however, perhaps better adapted to the purposes of an elementary teacher than those of the present day, which deal less with the differences which strike the eye of a student.

It is difficult to select points for notice in a work so concisely written; but we may mention two, taken at hazard. Ulcers which do not tend to heal are divided into three classes: "1. Ulcers which are prevented from healing by defect of action. 2. Ulcers which are prevented from healing by excess of action. 3. Ulcers which are prevented from healing by peculiarity of action." Again, mammary tu-

mors are classed as "simple, fibrous, cystic, carcinomatous, and medullary sarcomata." In regard to plans of treatment, also, the work before us is not fully up to the times. So far as it goes it is excellent; but it needs to be supplemented by other and more modern writings.

The monographic portion of the volume is, as might be supposed, more entirely satisfactory. Besides the papers mentioned in the title-page, there are here to be found numerous clinical observations of the highest value. Among these we are particularly struck with the cases reported of amputation at the ankle, of excision of the entire scapula, and of excision of the tongue. They are all, however, deserving of attentive perusal, and it is matter of congratulation that they have been collected in a permanent and attractive form.

Mr. Syme's name is, perhaps, most closely associated with the method proposed by him of amputating at the ankle, by sawing off the malleoli, the posterior flap being formed of the skin of the heel. This was extensively adopted in this country, and with great success, and is better suited to very many cases than any other procedure. Of late years, however, Pirogoff's operation, the os calcis being sawn through obliquely downwards and forwards, and its cut surface brought up against that of the tibia, has been more fashionable. Our own experience would lead us to regard the two plans as of equal merit; selecting one or the other according to the special case to be dealt with.

Of excision of the joints for disease, Mr. Syme is an ardent advocate. In America this practice has not perhaps been sufficiently tested. Our impression is that the number of these cases in this country is smaller than it is abroad. The shoulder, elbow, and knee are the joints in which the best results have been attained. Mr. Syme very justly objects to the operation at the hip, on account of the frequent involvement of the acetabulum, and thinks that a favorable issue in disease of the wrist and ankle is of such exceptional occurrence that amputation is preferable when these joints are concerned.

The external incision for the relief of urethral stricture, an operation original with Mr. Syme, is discussed not only in the general portion of the work before us, but also in one of the monographs in the appendix. It is undoubtedly the easiest method of dealing with many cases, and sometimes offers the only chance of safety for the patient. Our author seems to have had better success in avoiding the subsequent occurrence of perineal fistula than would be supposed possible; his recorded cases look so favorable that many surgeons will be apt to

imitate his practice. The plan should not, however, be indiscriminately employed.

In the foregoing brief notice, we have tried to present the impressions made upon us by the examination of this excellent addition to medical literature, with fidelity. To sum them up, we may say, that while it would not answer as the sole surgical guide of the young practitioner, it contains matter of the greatest value and importance to any one who would acquaint himself thoroughly with the views of the leading men of the century.

The typography of the volume is not only elegant, but remarkably correct; and the monographs are here and there illustrated by means of wood-cuts of great merit.

The Diseases of the Ear—their Diagnosis and Treatment. A Text-Book of Aural Surgery in the form of Academical Lectures. By DR. ANTON VON TRÖLTSCHE, Aural Surgeon and Lecturer in the University in Wurtzburg, Bavaria. Translated from the German and Edited by D. B. ST. JOHN ROOSA, M.D., Assistant Surgeon in the N. Y. Eye Infirmary.

Any new book at present appearing on the subject of ear diseases, must be carefully examined by our profession, as in that branch of surgery clear ideas of pathology and treatment are especially desirable. Among the authors in the English language, Wilde and Toynbee have undoubtedly done most for the advancement of aural surgery. The books of these gentlemen are at present difficult to procure, and are somewhat too voluminous to be carefully read through by general practitioners. The work before us contains in all but 254 pages, and comprises the diagnosis, pathology, and treatment of diseases of the ear. In its clearness, simplicity, and brevity it differs much from monographs generally. Technicalities are not numerous, and an extensive knowledge of the subject treated of is not presupposed.

We shall try to notice the chief points wherein this work differs from others of its kind, and to state any variation from formerly received opinions.

The author explains the modes of examining the external ear—comparing the advantages of *direct* and *reflected* light—and giving the preference to the latter. As a reflector, Dr. T. uses an invention of his own: a concave, silvered mirror of about six inches focus, with a central perforation for the surgeon to see through. The patient is placed *between* the surgeon and the source of light, (window or lamp,)

an ordinary ear speculum is introduced, and the mirror being placed before the eye of the surgeon, it is so directed that the light shall be reflected through the speculum upon the membrana tympani. This means avoids the necessity of a bright day for examinations; the surgeon's head does not cast a shadow in the speculum. For cleaning out the ear previous to examinations or applications, a syringe, or "angular forceps," and cotton, may be used; while using these forceps, or *any instrument*, we are cautioned to have the meatus illuminated by direct sunlight, or, better, by the mirror attached to the forehead.

A chapter is devoted to *ceruminous* collections, etc., which we are advised to remove by *syringing*. Foreign bodies should be dislodged in the same manner, very serious injury being often caused by attempts at their removal with instruments, especially when these are used without proper illumination.

Otitis externa, very often called *scrofulous*, is declared to be a simple *local* disease, often caused by an extension of skin disease or a foreign body in the meatus, and it will be cured by local treatment—the term "scrofulous" being merely a cloak for ignorance of its pathology. This disease often results in "otorrhœa," which is not a disease itself. In treatment of otitis externa, or other inflammation of the ear, if poultices be used, we are in danger of perforation of the membrana tympani. For relief of pain we are advised to pour in warm water frequently, leaving it in the meatus about five minutes at a time, or to hold the ear over steam. *All applications to the ear should be warm.*

Writers speak of "myringitis" as a frequent complaint. Dr. T. considers pure myringitis a very rare disease. The membrane of the tympanum generally becomes inflamed by the extension of inflammation from the meatus, or from the middle ear. Acute myringitis he has observed on *one* side only. Now pure myringitis would most likely arise from some injury or exposure affecting *one* membrana tympani. If, however, the disease described as myringitis be an accompaniment of inflammation of the middle ear, we know it would be likely to affect *both* membrana tympani, as it is ordinarily described as doing. The prognosis in myringitis is stated as favorable; even perforations of the membrana caused by *this disease* heal readily.

In regard to the use of the Eustachian catheter, Dr. T. speaks approvingly, except in case of children, where the width of the Eustachian tube does not render it necessary. It may be used advantageously where there is a collection of mucus or pus in the middle ear, and in closure of the Eustachian tube. It is useful both for di-

agnosis and treatment. By its means we may inject various vapors into the middle ear. The dangers of its use have been overrated.

Various instruments recently introduced are described and their uses stated: Toynbee's "otoscope" for auscultation of the middle ear, Eustachian tube, &c.; Politzer's "tympanimeter," for showing the amount of mobility of the drum; "Tröltsch's pump," for throwing vapors into the middle ear through the Eustachian catheter. *Liquid* injections are not spoken well of. Diseases of the *middle ear* are very fully discussed. Acute and chronic catarrh, and acute and chronic purulent inflammation, are each separately examined; their pathology and treatment are given at length. So-called myringitis is generally an inflammation of the middle ear, in which one part of the process—inflammation of the drum—is taken for the whole. Catarrh of the middle ear almost always occurs on *both* sides; it comes suddenly; it is accompanied by increased pain at night, and in swallowing; it does not hurt to move the auricle; noise in the ear is rarely wanting in this disease, hence, and on account of accompanying vertigo, &c., there is supposed to be disease of the brain and nervous deafness. In acute catarrh of middle ear, on examination, we find the auricle and meatus externus normal, the drum reddened or infiltrated, the "light spot" absent. The handle of the malleus often remaining visible, shows that the inflammation is deeper than the membrana tympani. The throat is found swollen or injected, "cold in the head" is often present, and other catarrhal symptoms. Among the bad results of this catarrh of the middle ear are, thickening of the mucous membrane of the cavity of the tympanum and adhesions between various parts, perforation of drum, &c.

The treatment advised is the *early* introduction of the catheter, local blood-letting, and a cathartic of calomel and jalap. If this does not ease the pain, keep warm water in the ear for fifteen minutes every hour; let patient stay in bed, and give diaphoretics.

Chronic aural catarrh often occurs without pain. Noise in ears, deafness, mental depression, feeling of pressure in the head, being the chief symptoms, and these are often placed to the account of "nervous deafness." The only *visible* signs of it being, sometimes a calcareous deposit on the membrana tympani, or a change of size in the "light spot." Post-mortem examinations often show thickening of the mucous membrane of the tympanum, extending to the articulations of the little bones, or to the membrana tympani secundaria, (membrane of fenestra rotunda.) This disease is often caused by extension of pharyngeal catarrh, or follows typhus and the exanthemata. In ear

affections it is always well to examine the throat, as the disease often extends thence. As in simple "catarrhal inflammation," Dr. T. divides "purulent inflammation" into acute or chronic. In its acute stage the disease, from its symptoms, is often mistaken for myringitis. In autopsies on infants, evidences of purulent inflammation were found remarkably often, they being absent in only thirteen out of forty-six ears examined. (These cases were among the lowest classes, very poorly nourished.) If the disease occurs so often among infants, it is fortunate for their membrana tympani that the Eustachian tube is very wide, else perforations would frequently occur. Among the results of purulent catarrh of the middle ear are mentioned post and pre-aural abscesses, pus in mastoid cells, &c. Trephining of the mastoid process has been performed several times successfully.

The existence of "nervous deafness" is doubted. As we advance in pathological knowledge, this, as well as other purely *nervous* diseases, is less frequently diagnosticated. Otalgia is considered a rarity, and, when occurring, as being often dependent on carious teeth.

"Deaf-mutism" is considered under three heads—"congenital," "early acquired," and "late acquired." The first is often attended by a partial or entire lack of semicircular canals. The second occurs in children who, answerably to their age, hear, but can not speak. The third comes later in life. The latter two often result from diseases of the cavity of the tympanum, when no abnormality of the internal ear can be discerned.

From the above abstracts it will be seen that "otorrhœa," "myringitis," and "nervous deafness" do not form the chief diseases of the ear in Dr. Tröltzsch's estimation. Several interesting cases and many notes have been added by the editor. It seems to us that a few pages on the anatomy (normal and pathological) of the ear would have been an advantageous prefix.

The translation in some places, we think, has been made quite too *literal*. As a thoroughly practical and very readable book, we consider this volume an important addition to the literature of aural diseases.

On Wakefulness. With an Introductory Chapter on the Physiology of Sleep. By WILLIAM A. HAMMOND, M.D., Fellow of the College of Physicians of Philadelphia, of the American Philosophical Society, &c. Philadelphia: J. B. Lippincott & Co., 1866. 12mo, pp. 93.

This monograph is an enlarged, and in parts re-written, essay "On

Sleep and Insomnia," published in the May and June numbers of this Journal. It is divided into four chapters. The first, Introductory, on the Physiology of Sleep; the second, on the Pathology of Wakefulness; a third, on the Exciting Causes of Wakefulness; and a fourth, on the Treatment of Wakefulness. The physiological views respecting the *immediate* cause of sleep, maintained by Dr. Hammond, are known to our readers, and are at variance with those of most physiologists. He claims that the doctrine of a diminished supply of blood in the cerebral tissues, during natural sleep, is established by exact observation; and that congestion of the vessels of the brain is the necessity in coma, a condition in which the causes and phenomena are distinct. The pathology of wakefulness, in connection with its exciting causes, is ably and sensibly treated of; and the influence of the wear and tear of our modern life in causing insomnia, with its frequent termination in mental derangement, and often in confirmed insanity, shown by convincing precepts and examples. Whatever difference of opinion may exist regarding Dr. Hammond's physiology, there can be none respecting his therapeutics. The treatment of wakefulness recommended by him is sound and rational, and must command the assent of every physician who has any experience with this distressing disorder.

1. *Report of the Council of Hygiene and Public Health of the Citizen's Association of New York, upon Epidemic Cholera and Preventive Measures.* New York, November, 1865. Pamphlet, pp. 48.
2. *On Epidemic Cholera, the Phenomena, Causes, Prevention, and Treatment, with an Appendix relating to the Brooklyn City Sewerage.* By NELSON L. NORTH, M.D., Surgeon Metropolitan Police, &c. Reprint from the Transactions of the Medical Association of the Eastern District of Brooklyn. 1865. Pamphlet, pp. 39.

The Report of the "Council of Health" of the "Citizen's Association" is by Dr. ELISHA HARRIS, an enlightened and practical sanitarian. In sketching the progress and aspects of the epidemic of 1865, a retrospect of the course and history of the four preceding epidemics is taken, with the view that the lessons taught by them may be made profitable. The present epidemic in Europe and Africa "appears to be as clearly connected with the epidemic which has been prevailing in India, as any former visitation of the cholera westward has been connected with the outbreak and diffusion of the epidemic from its oriental birth-place." It has followed, too, the laws established in previous

epidemics, as regards the physical and hygienic conditions that precede and exist wherever cholera elects its principal seats of prevalence and mortality. The usual forerunners of the scourge, it is claimed, have heralded its advent. In spite of the rigorous enforcement of quarantine regulations in the ports of southern Europe, cholera has pursued its regular march. The practical value of sanitary works—cleanliness, public and personal, ventilation, drainage, disinfection of foul places—has been constantly shown in the progress of the epidemic; and the consoling deduction can be safely made that while Asiatic cholera is, like other epidemics, “subject to certain general laws of diffusion and progress by atmospheric influences, some of the causes of which are not wholly subject to human control, we should also bear in mind that those causes which give to cholera a *pestilential* and extensively destructive character, *are known and preventable*.”

In all the previous cholera epidemics in New York, we find that the cholera fields and the fever haunts are the same. This holds true of London and other towns in Great Britain. The *localizing causes* of cholera are found to be: 1. Decaying organic matters, bone, hide, fat and offal houses, neglected stables, putrescent mud and filth. 2. Bad drainage, local dampness, malaria. 3. Obstructed sewers, filthy streets, gutters, stables, garbage, and cesspools. 4. Water and beverages in any manner contaminated by putrescent organic matter, particularly by any soakage from privies. 5. Neglected privies and putrefying excrements. 6. Overcrowding and neglect of ventilation. These have always been the most active of the exciting causes of the pestilence, and are all obviously controllable, and should be prevented and removed by authority. The conditions necessary for domestic and public health can not be disregarded by municipal bodies. A duty is involved which can not be shirked or unheeded without criminality, and should be insisted on. One of the particular duties indicated in this report is district and house-to-house inspection. The experience of the two last cholera epidemics in England has demonstrated signally the usefulness of house visitations by skilled medical men, and their practical value was shown in this city during last winter by the sanitary inspections made under the direction of the “Council of Hygiene,” when typhus fever and small-pox were prevailing. Certain restrictions and provisions for the segregation and proper care of persons arriving in vessels on board which there are cholera cases, are obviously reasonable enough; but it is absurd to hazard the protection of a city *exclusively* upon any quarantine restrictions. The well should be immediately separated from the sick, and the latter removed

and properly cared for; prompt measures should be taken to prevent the spread of the pestilence by means of the clothing or evacuations of the infected; and the vessels should be cleansed and disinfected without loss of time. But public attention should not be diverted from the chiefest sources of danger: 1. The ejected fluids and evacuations, and besmeared clothing of those ill with cholera. 2. The overcrowding of human beings, whether on shipboard or in unventilated houses. 3. Filth, dampness, and foul air. These exciting causes of cholera must be provided against by appropriate sanitary regulations and unceasing vigilance on the part of the public authorities.

Dr. Harris has appended to his report some valuable notes on cleansing and disinfection.

In Dr. North's essay we find nothing deserving of special notice. If it succeeds in directing the attention of the authorities of our sister city to "making preparations to meet this terrible enemy in his onward march towards us," it will have done good service, for the picture he gives of the state of things in Brooklyn directly bearing on the incubation and propagation of pestilence is by no means reassuring to the inhabitants or encouraging to the property holders. He says:

"Our streets are *filthy*, our tenements overcrowded, our sewerage, what there is of it, is but a miserable apology for the thing it should be, having a stinking reservoir for filth at every street corner, which, when it becomes too bad, is dipped out and carted off through the streets because the sewer-pipes are not of sufficient size to allow any thing but the strained water to run off through them. And our olfactories meet the stench from pig styes, and goat pens, and hide tanneries, and fat and slaughter houses; and even the bone-boiling establishments can hardly be said to be out of town, and one can scarcely tell, in fact, why it is that we have not always some epidemic upon us."

PROGRESS OF THE MEDICAL SCIENCES.

OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN.

1. *On the Facilitation of the First Stage of Labor.* By ANDREW INGLIS, M.D., F.R.C.S.E. Read before the Edinburgh Obstetrical Society.

At present many different means are in use for favoring the progress of the first stage of labor. Simple dilatation with the finger has from

time to time been alternately praised and decried. Just now the voice of the majority is in its favor; and I am certain that it does good in some cases, but only where the resistance to dilatation is not excessive. Within the last few years, india-rubber dilaters have been tried pretty extensively, and they also have been found of service. However, not only is considerable difficulty experienced in their use, but that difficulty is usually great just in proportion to the urgency of the case. Sponge tents also have been used in such cases, but I have never seen them produce any great result. While these our direct surgical means are so scanty, there is no lack of therapeutic agents which have been tried. Injections have been used extensively, both per vaginam and per anum, consisting of warm water, as well as containing opium, belladonna, and other drugs. Medicinal applications to the same parts, in a more concentrated form, have also been used.

Remedies intended to act through the general system are frequently given. Chloroform, which is the most valuable of these; opium, which also certainly sometimes does good; tartrate of antimony, bleeding, emetics, purgatives, and ergot, have all been in turn pressed into the service; but I think little need be said about their general inefficiency. My experience supports what I believe to be the general opinion—that it is precisely where dilation without interference is most tedious, that the foregoing means are most nearly inert.

To show that there is good reason for believing that another means—namely, separation of the membranes for some distance round the os—will promote dilatation much more efficiently than any hitherto proposed, is the object of this paper; and I believe that where attempts at direct dilatation have been successful, the success has been in great part owing to unintentional separation of the membranes. In pursuance of my object, I shall call attention to the effect on the character of labor of such a separation, either as occurring spontaneously or effected artificially for its induction or facilitation.

In the first stage of labor coming on and proceeding without interference, there are two opposite conditions of the passages—one in which there is a copious discharge of viscid mucus, and which is often called a “wet labor;” and another, in which there is hardly any, and labor is called “dry.” At the full time, the first seems to occur normally in the cow, mare, bitch, etc., and, I am inclined to believe, is natural also in the human female. I consider the following as being the natural process in women: The ovum having become ripe, the membranes separate from the cervical portion of the uterus, if not from the whole surface. They then by their weight press more heavily against the cervix, even when the patient is lying down; and as the pressure is soft, equable, and continuous, the cervix gradually yields to it and becomes quite slack, and this takes place without the occurrence of pain. Next, when relaxation has become complete, the mucous discharge commences, proceeding from the uterus. Finally, a pain comes on and terminates the first stage. That the discharge comes from the uterus is shown by its protruding from the os previous to its appearance in the vagina. Besides, it is only found when the membranes are already separated, and is very often tinged with blood before pains have been felt. A process resembling this form of the first stage may be seen where the other muscular canals are concerned, and perhaps most prominently in the case of the rectum. If

the finger or bougie is gently and cautiously inserted past the sphincter ani, and kept there for some time, complete relaxation gradually ensues, a profuse discharge from within the sphincter comes on, and, if the bougie is allowed to remain long enough, the muscular fibres above begin expulsive action.

The foregoing explanation seems equally applicable to "wet" cases of abortion or miscarriage in the human subject, except that in such cases health ripening of the ovum can not be said to be the cause of the separation of the membranes.

The other form of the first stage, which I have designated "dry," is very different. Preliminary to labor there is no separation of the membranes, and consequently no slackening of the cervix, and there results a great increase in the amount of uterine exertion required, even where nothing worse is entailed; for, though separation and dilatation generally at last take place, they do so only at the expense of multiplied and more distressing pains. At best, the pains may stop for a time after causing some separation, and labor on their return may thereby then come to take on the "wet" form; but very often no such intermission occurs, and dilatation costs a long series of wearing-out pains. Moreover, the membranes may rupture, requiring dilatation to be effected by the fœtus, and therefore even slower and more painful. Again, sometimes the imperfectly dilated cervix is forced into the pelvis along with the head, and becomes jammed between the two, increasing greatly the pain and delay. Lastly, the resistance to dilatation may cause permanent cessation of the pains, or the cervix, or body of the uterus, or both, may rupture.

For some time, when first in practice, I was in the habit of inducing premature labor in cases requiring such interference, either by introducing a catheter between the membranes and the uterus and leaving it there, or by using sponge tents. In these cases, labor often came on very soon, though a few resisted for a long time the action of both means combined, and the membranes had to be ruptured before pains could be made to come on regularly; but in all cases, whether labor followed at once or came on only after great expenditure of time and trouble, the first stage was slow and painful, and in some the pains continued for days before dilatation was complete.

About eighteen months ago, I attended a case where Dr. James Sidney brought on premature labor by the late Dr. Hamilton's method. By means of Dr. Hamilton's uterine bolt, he separated the membranes for some inches round the cervix, and the instrument was passed in and turned around every twelve hours. About forty-eight hours after the first separation of the membranes, the os was soft and dilatable, and a little discharge was apparent at the top of the vagina. Six hours later, I found the discharge copious; a few minutes after I had ascertained this by examination, labor came on suddenly; and in three or four pains the first stage was completed, and the head well down in the pelvis.

Soon after this I induced premature labor in the same manner in another case. In about forty-eight hours the discharge was copious, and the os soft and dilatable, but no pains had appeared; but immediately after my examination one pain came on and completed both stages.

Since the first of these cases I have never employed any other means for inducing premature labor, being convinced that it is the easiest for the patient, though not always the speediest. The first stage of labor induced by it seems identical in all respects with that of the "wet" form of labor occurring spontaneously.

The circumstance of the first stage of labor so induced, presenting so favorable a character, has led me to use that separation also for the facilitation of labor already begun in cases rendered tedious by so-called rigidity of the os. The number of cases in which I have had recourse to this mode of procedure is perhaps not yet sufficiently considerable to entitle me to lay much stress on these as bearing on my argument; but I may state that all of them which can be considered as affecting the question seem to support the practice I am advocating. The following are particulars of a few:

To recapitulate—

1st. The easiest form of the first stage of labor is characterized by protrusion of the membranes and a copious discharge.

2d. These are always direct consequences of separation of the membranes.

3d. The result of artificial separation seems precisely similar to that of spontaneous.

The following is the practice I would recommend in regard to separation of the membranes: 1st. That it should always be the initial measure in the induction of premature labor; and that until complete relaxation of the os has resulted from it, there should be no further interference of any other kind; 2d. That when labor has begun without previous separation of the membranes, and these are still adherent, they should always be at once separated, as the best means of overcoming the rigidity of the os, and the painful and prolonged first stage, which almost invariably accompany such a state of matters.

In the latter of these two cases, if the pains are severe and ineffective, I should be inclined to recommend sedatives to be given at the same time, in the hope of gaining time for painless relaxation to take place.

With regard to the means of separating the membranes, I have in most cases been able to do it with the finger, though in one or two an instrument was required. In one case, which I have not reported, I could not at first get the finger far enough in to effect any extensive separation; but the small portion round the os, which was thus denuded, was so relaxed an hour after that I was able to insert the whole finger and separate to the desired extent.—*Edinburgh Med. Journal.*

2. *The Management of the Third Stage of Labor.* By Dr. H. EASTLAKE.

Having briefly described the opinions which existed amongst the accoucheurs of times gone by, the author proceeds to give an account of the modern views and principles which govern the placental stage of labor. Dr. Eastlake lays great stress upon the hand being placed firmly on the fundus uteri at the moment the child is being expelled, the uterus being thus followed down, and the contraction maintained by gentle pressure. He states that external manipulation, judiciously applied, was, in the majority of instances, quite sufficient *per se* to effect the expulsion of the afterbirth, without any traction whatever on the funis. He believes that the great secret is to exert the pressure during a contraction; in short, to act in unison with nature as we did in the application of forceps, where we applied our chief force at the moment of a pain. Dr. Eastlake says he had no doubt that many would imagine that, after all, this was no modern idea; but he demonstrates that this teaching was not definitely described and insisted on in our manuals of obstetrics. Dr. Credé, the Professor of Midwifery at Leipsic, appeared to be the only one who had advocated this doc-

trine and brought it prominently before the profession. The author next considers the subject of retained placenta, and alludes to the various causes which arrest nature's process of extruding the after-birth. The three steps in the natural expulsion—namely, (1) the detachment from the wall of the uterus, (2) its extrusion from the uterine cavity, and (3) its expulsion from the vagina—are duly recognized and dwelt upon. Regarding the subject of morbid adhesion of the placenta, Dr. Eastlake throws out a suggestion as to the possibility of being able to diagnose this condition by means of auscultation. He reasons by analogy in stating that for a long time he had been fully persuaded that by means of auscultation we often possess not only a negative but a positive sign of foetal death. He describes a peculiar modification of the uterine *souffle*, which to his ear was very characteristic when foetal life had been extinct for any time. The alteration in tone suggested the idea of a muffled sawing noise, very different to the gentle blowing murmur heard in normal cases, where a living child existed in utero. How soon the modification took place the author is unable to state, from want of sufficient field for observation. He considers that no ergot of rye should be given in cases of retained placenta, unless we were quite sure that no abnormal adhesion or irregular contractions existed. In cases of spasm of the os uteri, where the placenta became encysted, the administration of chloroform is recommended. Another point of interest alluded to by Dr. Eastlake, bearing upon the subject of his paper, is the occasional existence of a supplemental afterbirth, which was spoken of by Dr. Barnes, Dr. M'Clintock, and other authors, under the name of *placenta succenturiata* or *placenta spuria*. Dr. Eastlake has seen a specimen of such an afterbirth in the museum of the Lying-in Hospital in Dublin, obtained from an ovum of five months. When such a mass remained in the uterus after the true placenta had been expelled, it often gave rise to secondary hemorrhage, and an impression arose that due caution had not been exercised in the extraction of the afterbirth. He agrees with Dr. M'Clintock that, remembering the possibility of such an occurrence, we should be slow to utter any opinion which would damage the character of a professional brother. In conclusion, the author alludes to the several conditions which generally authorize us to have recourse to a speedy removal of the placenta, such as postpartum hemorrhage, convulsions, rupture of the uterus, and possibly, under certain circumstances, where the uterus was inverted, with the afterbirth still adherent.—(*Proceedings of Obstetrical Society of London.*)—*Medical Times and Gazette*.

3. *On Dilatation of the Os Uteri during Labor by Incisions.* By Dr. H. HILDEBRANDT, of Königsberg.

The author commences by a brief account of nine labors, in seven of which primiparae, advanced in life, suffered from rigidity of the os uteri; against which ipecacuanha, opium, poultices, baths, bleedings and chloroform were all unavailing. Incisions were made, after which all the cases were fortunately terminated. Incisions were also made, with a like favorable result, in one case of convulsions, and in one of prolapsus of the cord.

He proceeds to consider the supposed risks that have deterred accoucheurs from the performance of the operation. It has been feared that the pain of incisions, in a part already irritated by foetal pressure, and in persons inclined to nervous disorder by prolonged

labor, might be productive of mischief. This fear is wholly groundless; the incisions themselves being scarcely felt by the patient, and the relief actually afforded by them being very great. Others have dreaded an extension of the incisions during pain, so that they might come to involve the substance of the uterus, and produce the fatal effects of rupture. This is visionary. The incisions do sometimes yield a little, but never so far as to reach even the cervical portion of the womb; and the operator, by relieving an impediment to the advance of the foetus, diminishes instead of increases the danger of rupture. Lastly, it has been feared that excessive hemorrhage might attend or follow the incisions, but this fear is never realized in practice. In cases that require such treatment, the os uteri is morbidly changed, and so bloodless that the hemorrhage from the incisions does not exceed a few drops. Where incisions are made into a healthy uterus, in order to effect rapid delivery, the bleeding may be greater, but its source is always accessible, and it may, therefore, always be readily controlled, while, in such cases, which are almost limited to eclampsia and placenta prævia, the danger from hemorrhage can never be equal to the danger of delay. The operation is chiefly indicated, however, in morbid conditions of the vaginal portion of the cervix, such as rigidity, hypertrophy, and malignant disease. For forced delivery, with a healthy cervix, the incisions should be six or eight in number, and not more than three lines in depth.—*Half-yearly Abstract of the Medical Sciences.*

EDITORIAL.

The record in relation to cholera at this port remains about the same as last month. We have no well authenticated case reported as having occurred beyond the vessels which have arrived in the outer harbor. All suspected vessels have been rigidly quarantined, and, when the evidences of disease have been marked, passengers with their clothing, the cargo and the vessel, have been subjected to a species of disinfecting fumigation by the health officers. This is one fact, and another is that the disease has not spread beyond the vessels and their living freight.

It is reported that the epidemic has made its appearance in the island of Guadaloupe, West Indies, not far distant from our Southern ports.

In Europe the excitement has more or less subsided. England has thus far comparatively escaped, notwithstanding the constant communication between it and the infected places of Continental Europe, of both travelers and articles of merchandise. Medical writers do by no means suppose that the epidemic has expended itself, but look for a fresh outbreak of the disease during the coming spring.

The total number of deaths in Paris, officially returned, from Oct. 1 to Dec. 3, inclusive, amounts to 6,011; of these 4,602 occurred in

October, 1,365 in November, and 44 in December; of the whole, 3,845 were in prisons and hospitals, and 1,835 in private hospitals; the rest are not accounted for.

Thus far the disease has shown itself in its most virulent form. We can give no better account of it, as it has prevailed in Egypt, along the Mediterranean coast and in Paris, than is portrayed in the communication to the *Medical Times and Gazette*, Nov. 18, of JOHN PATTERSON, M.D., of the Egyptian medical service, *On the late Pestilence in Cairo*, from which we make the following extracts:

In its most fatal forms the disease presented the following characters: Sudden and acute pain in the epigastrium, with a feeling of great nervous depression and chilliness; a large bilious motion suddenly passed, frequently, but not always, followed by a large watery motion within a few minutes, having mucous flocculi floating on it; nausea, with or without vomiting, the vomited matter being the same as that of the last motion. The bladder was emptied with the first motion; afterwards no urine was passed. In the worst cases no more purging or vomiting; but if the latter continued, it was not at all spasmodic, the patient simply ejecting the fluid from the mouth in a full stream, and without effort; pulse, from the first, scarcely perceptible; tongue white and cold; breath cold; voice husky and weak; skin covered with a viscid moisture, which often appeared suddenly and at different stages of the malady. In an hour, or less, dead collapse set in, and the patient expired within six or eight hours, showing great restlessness. In all the cases the mental faculties were perfectly clear to the very last moments. In many of the cases the collapse was more sudden; the patient was struck down as if by a blow; there was neither purging, vomiting, nor cramps—all the functions were suddenly suspended, yet the intelligence remained perfect. Of thirty-four deaths in this class, more than half died within eight hours; the longest period was eleven hours, only excepting three or four who, after coming out of collapse, died from exhaustion and want of functional power. A second mode of invasion was equally well marked: Sudden purging, and vomiting; mild cramps of the leg and abdomen; great oppression of breathing, and feeling of heavy weight at epigastrium; marked diminution of the pulse; coldness of the surface of the body; restlessness and anxiety. The Skin assumed a dark leaden hue, which was wanting in the more sudden cases. Collapse gradually came on. The purging and vomiting seemed always to cease in these cases just as the collapse was fairly established, or a little before; no secretion of urine. This form was as fatal as the first, only a few hours longer continued, death generally occurring within twelve hours; intelligence also perfect.

The third form of invasion varied somewhat. Cramps were generally the first symptom, more or less violent; the purging and vomiting severe, immense quantities of a whitish, whey-like fluid being passed, with mucous epithelium floating on it; function of kidneys suspended.

In all these cases engorgement of the liver was more or less manifest; the organ itself could often be felt distinctly, and seen bulging much beyond its natural limits. The pain in the epigastrium was not severe in these cases, nor was the sensation of weight much complained of. The collapse, though well pronounced, did not partake of the same deadly characters as in either of the two former modes of invasion. The greater number of these cases recovered from the collapse; many, however, died from congestion of the brain and other organs, and in all the convalescence was complicated by local congestions. In one case no urine was passed for five days, yet the patient recovered. Several had severe jaundice.

These are three well marked modes of invasion observed in a varied practice amongst almost every variety of the human race. Many cases, of course, could not be referred to any distinct class, yet one circumstance marked a great difference between this epidemic and a previous one in Egypt in 1855, viz., that premonitory diarrhœa was not a distinctive character of the disease. The majority of the cases had no premonitory diarrhœa; they were men and women struck down in the prime of life and in good health.

Post-mortem examinations have thrown no new light on the true nature of this malady,—pulmonary and hepatic engorgement; heart loaded with black, thick blood; stomach and intestines generally bloodless, containing more or less of a thin milky fluid; a glairy sort of mucus adhering to the mucous membrane of the intestinal canal. The membrane itself had a sodden, œdematous appearance, and could be as easily separated from the subjacent tissues as the rind from a mandarin orange; the kidneys healthy; urinary bladder firmly contracted. The medicines were found in the stomach as they had been administered a few hours before, clearly proving how little good they could do in such cases. As a rule, the secondary fever presented a marked typhoid character, not admitting very active treatment. It was more severe in the cases where the collapse had been milder, but longer continued; more active where the collapse had been sudden, and of shorter duration. The most marked symptom during recovery from true cases of the pestilence was the enormous quantity of black, oily looking matter passed by stool.

-- More medical journals are announced in the South. Dr. Frank Ramsey, of Memphis, Tenn., has issued a prospectus for a *Medical and Surgical Monthly*, to be published in that city. Dr. Bennet Dowler purposes to revive the *New Orleans Medical and Surgical Journal*, of which he was for many years the editor. It is rumored that a fortnightly medical journal will soon appear in this city. Its title has not been announced. It is said that Dr. Geo. F. Shrady will be the editor.

-- The attention of the Alumni of the College of Physicians & Surgeons is called to the advertisement in this issue of the *Journal*.

-- Dr. Richet succeeds Prof. Malgaigne in the Chair of Surgery at the Paris School of Medicine, made vacant by the death of the latter.

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FEBRUARY, 1866.

ORIGINAL COMMUNICATIONS.

Paralysis from Peripheral Irritation, with Reports of Cases.
By S. WEIR MITCHELL, M.D., Philadelphia.

The writings of many of the older physicians, as well as those of a later day, contain numerous cases of palsy, local or general, which, upon cadaveric section, revealed no material lesion. To such cases various names have been affixed, with the intention of grouping a set of unexplained facts, or of indicating by the nomenclature the writer's view of the mode in which the malady was produced.

It thus happens that the cases referred to have been classified under such terms as idiopathic paralysis, essential paralysis, functional paralysis, inhibitory palsy, paralysis by exhaustion, reflex paralysis.

The industry of modern clinical observers has increased the number of these cases, and it has gradually become apparent that they differ very widely as to their original causation, and perhaps also as to the condition of the neural organs which corresponds to the various groups.

Of late, however, the temptings of an ingenious theory, set forth by a neurologist of great ability, has led many physicians

to follow him in a view of these cases which unites them all in one single class, whose nomenclature is meant to point out the pathological mechanism which gives rise to their occurrence.

Numerous cases of paralysis without apparent lesion, are to be found scattered throughout the older writers, as Forestus, Willis, Abercrombie, Bretonneau and Ollivier. Whytt,* who gave larger development and a new direction to the theory of sympathies held by Willis and others, does not report any case of palsy as due to sympathy.

In the year 1833, Mr. Stanley published a paper, entitled, "On Irritation of the Spinal Cord and its Nerves, in connection with Disease in the Kidneys." (*Med.-Chir. Tr.*, vol. xviii., p. 260.) He gives instances in which "irritation, commencing in the nerve of an internal organ—the kidney or bladder—has been transmitted through the spinal cord to the motive and sentient nerves of the limbs," so as to cause paralysis of the legs.

About the same time, or rather later, Dr. Graves reported a set of cases of paraplegia due to intestinal and other enteric maladies. Here, for the first time, the term reflex is used as applied to palsies occasioned by a morbid cause acting on some peripheral part of the body.

In 1841, Rayer (*Maladies des Reins*) reported a number of paralyzes without observed lesion, and due, as he supposed, to pre-existent disease of the genito-urinary organs, but notably of the kidneys. He did not theorize at all upon the mode of their production.

Important sets of cases, presumed to be paralysis without lesions, and of peripheral origin, are to be met with in papers by Leroy d'Etiolles, 1857, Landry, 1855, and Macario, 1857. The principal literature of the subject is completed when we enumerate the two essays by Brown-Séquard, 1860 and 1861,

* On page 165 of Brown-Séquard's *Lecture on the Physiology and Pathology of the Cerebral Nervous System*, Phila., 1860, he states that Whytt, on *Nervous Diseases*, etc., p. 18, quoting Hildanus, records cases of paralysis caused by pressure on a sensitive nerve or by wounds. I have been unable to discover this reference. Whytt does not give any case of sympathetic paralysis, although to sympathy he attributes a great number of other morbid phenomena. Jacoud, p. 342, also ascribes to Whytt the mention of sympathetic palsies.

in which he used the term reflex paralysis, attributed it to contraction of spinal vessels, induced by peripheral stimulus, and claimed, as thus created, a vast number of cases of palsy, including the bulk of those previously reported as without lesion.

This work called forth a brilliant critical paper by Mr. Gull, (*Med.-Chir. Tr.*, vol. xviii.,) who had already objected, in 1856, to the urinary paraplegia of Stanley. Dr. Brown-Séquard has also been met with objections by Nasse and Vallenin, whose papers I regret not to have seen.

I propose in this present essay to examine, as far as possible, the sets of cases attributed to peripheral causes acting through afferent nerves, and so ascertain which of them it is reasonable to presume may have been otherwise brought about than in the manner just mentioned. I shall then consider with care the etiology of those cases which still remain to us, with a view to determine whether they are due to reflex paralysis of spinal blood-vessels,* or to over-excitation and exhaustion of the central ganglia,† the two views which are now most favorably entertained by neurologists.

The following varieties of paralysis have been claimed as of peripheral origin:

1. Paralysis arising during diseases of the genito-urinary organs.—(Stanley, Graves.)
2. Those which occur during or just after dysenteries, diarrhoeas, super-purgation or in connection with worms.
3. Such as arise during or after pneumonia or pleurisy.
4. Such as are seemingly brought on by dentition.
5. The paralysis of diphtheria, fevers, eruptive disorders.
6. Such as seems to be occasioned by cold or by cold and moisture.
7. Paralysis due apparently to external injuries.

All of the above named cases have been thought to be owing to a peripheral impression setting out from some organ or part, and in some manner affecting the nerve centres more or less largely, so as to occasion loss of motion or sensation, or of both at once.

It is impossible, however, to admit that all of the cases which

* Brown-Séquard.

† The common opinion. Gull, Jaccoud. *Med. Circ.*, No. 6, *op. cit.*

we have for convenience grouped together were produced by peripheral irritations. It will, therefore, be requisite to scrutinize their pretensions and to reject such as are more reasonably to be explained in any other manner.

In the criticisms which follow I have received valuable aid from the excellent work of Jaccoud on paraplegia, and from the admirable essays of Dr. Gull.

Class First.—Paralysis arising during disease of the genito-urinary organs. Since much that I have to say in regard to this group has a wide critical application to members of the other classes, I shall dwell upon it with especial care.

It appears to me that both Gull and Jaccoud are correct in their idea that very many cases of apparent palsy from vesical or renal disease reported by Leroy d'Etiolles, Jun., and also some of the reputed instances of paraplegia from uterine affections, were not entitled to be looked upon as other than instances of such feebleness of the legs as we often meet with in long continued disease of the pelvic organs.

As Dr. Gull remarks, many of Leroy d'Etiolles cases which have the heading "Paraplegia," show no further evidence of this condition than such terms as "faiblesse dans les jambes," "faiblesse des membres, telle qu'elle ne permet plus au malade de se tenir debout sans appui," &c.

"If," he adds, "we bear in mind how much the activity of the lower limbs depends upon an unfettered action of the abdominal and lumbar muscles, and of the *psoæ*, *iliaci* and *glutæi*, we can not but recognize a fertile source of 'pseudo-paraplegia,' in that impairment of muscular activity which necessarily attends the malaise of chronic pelvic affections, whether vesical or uterine."

Let us also recall the fact, that the members whose weakness was made a test of palsy, are called upon to support the trunk constantly, as well as to perform locomotion, and we shall be well able to conceive that a person, desirous of making the worst of his cases, might find in these patients enough of excuse to enable him to label that as palsy which was in reality but simply general weakness most frankly expressed in the lower limbs, as indeed want of power nearly always is. That it is not in every instance easy to decide as to whether or not excessive feeble-

ness be paralytic, is best shown after diphtheria, where, expecting paralysis to occur and finding great weakness, we are puzzled as to its true nature.

One other fact of general interest and application calls for notice. It is impossible to read thoughtfully the many cases of paraplegia originating during or after visceral disease, without suspecting that in some of them the disorder of the nerve centres has preceded the malady which has been supposed to occasion it.

Dr. Gull has stated this point with great ability, pointing out the dependence of the viscera upon the sympathetic nerve, and of this upon the spine. No fact in neurology is to-day better established, than the readiness with which the viscera become diseased after their sympathetic nerves have been injured. Every physician has seen paralytic affections preceded by functional trouble of gastro-intestinal and other organs. When we consider these statements in connection, we can understand how the effect may be taken for the cause, the child for the parent.

The cases of Mr. Stanley and Dr. Graves have acquired an almost classical value, as authority for the existence of certain of the forms of peripherally induced paralysis, and their etiology has been scarcely questioned until Dr. Gull, and more lately M. Jaccoud, examined critically certain of those which seemed to them the weakest.

Mr. Stanley's paper, *op. cit.*, begins thus: "In the following paper cases will be related of disease in the kidneys existing in connection *with tenderness of the spine*, and paralysis of the lower limbs," &c. On turning to Brown-Séquard's table, symptomatically defining urinary paraplegia, we find that pains in the spine, either spontaneous or from pressure, shock, heat or cold, are very rare." So that what Mr. Stanley defined as a general feature of his cases, is just what Dr. Brown-Séquard declares to be very rare in this particular class of patients.

The latter authority also describes anæsthesia as uncommon in urinary paraplegia. I find that in four of Stanley's seven cases sensation was lost or much lessened, and in four others, of which he gives a brief account, and for which Mr. Hunt is responsible, there was numbness.

It will be worth while to examine in detail the cases which Mr. Stanley reports.

No. 1 was admitted with paraplegia, combined with retention of urine. Sensation and motion were lost. Autopsy, abscesses of one kidney, engorgement of the other. No disease of cord or brain.

Here there is absolutely nothing to show that the paraplegia did not precede the renal troubles, and therefore no reason to ascribe the paraplegia to the diseased kidneys.

No. 2 had partial loss of power in his upper and lower limbs, irritation of bladder, and occasional retention of purulent urine. Autopsy, small renal abscesses on one side, diseased bladder, spine and brain healthy. Here again we are unable to say which came first—the disease of the genito-urinary organs or the palsy. It is also remarkable that he should have had so extensive a paralysis. In this, as in most of these cases, the previous history is incomplete, and not a word is said as to any of the many constitutional causes which might account for the conditions present.

This is the more notable, because, in general, diseases of the male genito-urinary organs arise from gonorrhœal maladies, and occur in just such persons as are most likely to be, or to have been, exposed to the syphilitic poison.

Case 3, æt. 22, had a fall and bruised his back, but is said to have recovered. Some weeks later he was chilled after exercising, which was followed by severe pain in the loins, relieved to a certain extent by cupping. The pain continuing, he lost sensation, motion and the power to retain urine. Autopsy, large abscess in one kidney, small ones in the other; no disease of spine or brain.

I fancy that most physicians would have been unwilling to refer this paraplegia to the renal malady. There is nothing to show that the kidneys were first diseased, and much in the history of the case to make us suspect that the fall was the primary cause of both maladies, or that the spinal disease and the kidney complaint were both due to the exposure to cold.

Case 4 had a fall, followed by paraplegia. There are no further particulars. Autopsy, abscesses of both kidneys, thickened bladder, enlarged prostate, brain and spine normal, except an unusual vascularity in the membranes of the latter, below the first lumbar vertebra. In this case renal

disease was not suspected during life, and there seems to me no difficulty in supposing that here, as in the last case, the fall caused the paraplegia. This at least would be the presumption.

Case 5 is given in fuller detail. Gonorrhœa, retention of urine, incontinence of urine, with increasing paralysis of motion and sensation in the lower limbs; death in two weeks. Autopsy, kidneys very vascular and full of minute purulent deposits, pus in their cavities, inflamed bladder, no spinal or cerebral disease. In the absence of information as to the man's habits and previous history, we can only affirm that the double maladies seem to have run a course together, and that it is impossible to feel confident that the spinal disease followed the secondary troubles, which grew out of the gonorrhœa.

Case 6 seems to have been an ordinary gonorrhœa in process of cure; sudden paraplegia ensued; death in sixteen hours. Autopsy, the membranes and substance of the lumbar cord were, to some extent, turgescient, and there were a few drachms of transparent fluid in the theca. Liver enlarged and indurated, both kidneys intensely congested, as well as the bladder and ureters; some fluid in the ventricles of the brain. The probability of there having been spinal disease in this case seems to me so very great, that it is likely that a microscopical examination would have revealed extensive lesions. I am not sure that it was not a case of peripherally induced palsy; but the strong likelihood of there having been undiscovered neural lesions, and the death having followed the paralytic attack in sixteen hours, make me hesitate as to the true pathogenesis.

Case 7 had for two years incontinence of urine and pain in the back, as well as difficulty in his breathing. The only paralysis was that of the vesical sphincter, until the approach of death, when he also passed fœces involuntarily. Autopsy, a considerable quantity of serum was found beneath the arachnoid membrane, covering the brain. Serum was also found in the theca vertebralis, and the pia mater covering the lumbar portion of the cord was very vascular. The kidneys were mottled, and their cavities, as well as the bladder, contained puriform fluid. In this case the utter want of dates

defies critical inquiry. Probably it was, at the close, a case of serous apoplexy, with precedent congestion of the membranes of the cord; at all events, we do not observe that the paralysis distinctly followed the extra spinal disease. In fact the loss of power of the vesical sphincter, was the only paralytic phenomenon present until near to the close of the case.

The cases of palsy here quoted certainly do not force us to conclude, that they grew out of the renal or other affection to which the author ascribes them. In some of them, there is distinct evidence of spinal disease, and I have little doubt that the microscope would have revealed still deeper and more general lesions. Without it such an examination would have, to-day, but little value. But, apart from this, it is also clear that the autopsies were imperfect. In none do the renal and spinal veins seem to have been looked at with care, and yet these are just the channels through which suppurative disease might be expected to propagate itself to the spine. In fact, this is exactly what has been seen to occur in such cases as Gull* and Kussmaul† have reported.

While, therefore, I am inclined to suppose it possible that renal or vesical disease may occasionally determine a loss of spinal power, I am unable to admit that the relation has been clearly made out in any single one of Stanley's cases, or that any were so thoroughly studied as to make it unlikely that they could have been induced by the material propagation of visible lesions, by the toxic activity of certain well known maladies, or by defects of nutrition, consequent upon a blood

* Dr. Gull. *Med.-Chir. Trans.*, 1856.

† Kussmaul. *Zur Lehre von der Paraplegia Urinaria*. (*Wurzbürger Med. Zeitschrift*, vi., 1863.) As I do not find this journal accessible, I quote, translated, M. Jaccoud's summary of Kussmaul's very important case. A man, æt. 58 years, became paraplegic during an attack of chronic cystitis. At the autopsy, Kussmaul found lesion of bladder, peritonitis by propagated inflammation, atheromatous degeneration of the two hypogastric arteries, and fatty transformation of most of the nerve tubules in the two sciatic trunks. The pretended functional paraplegia was then an organic paraplegia of peripheral cause. Two conditions had produced it—altered nerves and altered arteries. The author remarks, with reason, that the latter malady, if severe, would alone account for the palsy, owing to the nutritive troubles in the sacral plexus, which might be thus occasioned.

altered by long continued previous disease of important viscera.

Dr. Graves' case of urinary paraplegia has always been confidently quoted. The subject was a sailor, who had suffered from a strain in the back, from gonorrhœa, excessive fatigue, great exposure to weather, and deprivation of usual stimulus. He had stricture, painful micturition, and increasing feebleness of the legs, resulting in considerable loss of motion. "A few days after the first introduction of a bougie, a remarkable amendment took place in his back and legs. In fact, it was almost sudden." Warm baths, frictions, etc., completed his cure, about a month after his admission to the hospital.

Except the sudden improvement after use of the bougies, there is nothing to point decisively to the urethral disease as the cause of the paralysis. The case reads more like one of exhaustion, aided or hastened by the weakening influence of disease; and in fact very nearly resembles a type of cases which our recent war produced, and which were familiar to the staff of the Hospital for Diseases and Injuries of the Nervous System. Dr. Graves is only responsible for this history from having adopted it from Dr. Hutton, who attended the patient. It is not a full account, does not tell us how complete was the palsy, and leaves untouched the question as to the condition of sensation, which may or may not have been lost, but which, according to my experience, is rarely deficient in persons who have incomplete paraplegia from over-exertion as a principal cause.

The cases reported by Rayer are also cited as palsies of peripheral origin. The first is quoted briefly from Ammon. (*Preuss. Medic. Zeitung*, 1832, No. 6.) An inflammation of the intestines was followed by obstinate constipation and hæmaturia, pain in the left leg succeeded, and paraplegia ensued. After death the left kidney was found enlarged and tubercular—a very suspicious fact, especially as no examination of the brain or spine seems to have been made. It is needless to criticise the case any further.

The second case was a chronic cystitis, with suppurative disease of both kidneys, followed by paraplegia and death

eleven days later. No lesion was found; but with Gull's, and especially with Kussmaul's cases before us, it is impossible to admit absence of lesion, without microscopical inspection of the tissues having been employed. As I shall point out hereafter, it is not improbable that cases of induced palsy from peripheral irritation may result in material lesions, yet other evidence of the pathogenesis of the case would be needed than was present in the instance before us. Perhaps I should add, that the body of the fifth dorsal vertebra contained a small tubercular mass. The surface of the spinal column opposite to this presented no peculiarity, although, as I have urged, the value of this statement is lessened by the want of an examination with the lens.

Case third was scarcely entitled to be termed paralysis. The limbs were feeble, and there was great pain in walking. It was probably a pseudo-paralysis.

In all of these cases—and the remark applies equally to most of Stanley's—there must have been more or less uræmic poisoning. I am not prepared to say how far this affected the result, as I have never seen in an adult this form of toxication determine distinctly a paralysis of the legs, although twice in the post-scarlatinal uræmia of childhood I have observed such a termination.

It would be needless to analyze the cases of other authors. As a rule they fail to exhibit the characters which Brown-Séquard has given, in tabular shape, as defining his conception of palsy from genito-urinary disease, and when subjected to more rigid tests as to their having indubitably originated from this form of peripheral irritation, they yet more signally fail.

From this condemnation I should except Dr. Echeverria's* singular case of temporary palsy from uterine irritation, and some of the reports made by Le Roy d'Etiolles.

Class second—Includes the paralyzes which ensue during or after dysentery, diarrhæa, or superpurgation, worms, &c. All of this class have been claimed as cases of functional paralysis from extra-central disease, or, as Brown-Séquard would label them, reflex paralysis.

* New York Medical Times, 1863.

We may start here with the admission that in rare cases worms occasion paralysis which is relieved by their removal. The few cases on record are more free from the chances of mistaken observation than the complicated histories with which we have last had to deal. In some, however, a fuller description of symptoms and of the after-history would have been desirable.

Before closing this brief discussion I will relate a case which may suffice to show that in the cases of central nervous disorder, apparently caused by worms, there may be a chance, at least, of a mistake. The case in question occurred several years ago, and was triumphantly noted at the time as an instance of reflex paralysis.

L. P., male, æt. six years, a strong, healthy looking child, born of parents one of whom, the father, had suffered from syphilitic accidents. The child teethed without trouble. In the fall of 1854 he had a malarious fever, which gave way under the use of quinia. He was not quite well from this date—September, 1854; but I could find no special disease. He was indisposed to move about, lacked appetite, and awakened often with a cry as though dreaming. A little later—about the end of October—he complained at times of pain in the stomach, and two days after passed a large worm—*A. Lumbricoides*. Upon this I laid aside the tonics he had been taking, and directed his mother to give him a tablespoonful of castor-oil, with twenty-five drops of turpentine. The evening of the same day, while being undressed, he complained of weakness, and, suddenly slipping down on the floor, had a very trifling spasm of the facial muscles, and became insensible. I was sent for at once, but did not see him until two hours later. At this time he was capable of being roused with difficulty, but on placing him in a hot bath he revived still further, so as to speak and ask for a drink of water. I now observed that he had a slight drooping of the left eye-lid, and that both of his legs were partially paralyzed as to motion. The want of power was nearly absolute on the right side, but the same defect was also sufficiently obvious in the other leg. Sensation may have been deficient. I can not be sure as to this point, since nothing is more difficult than to determine in children the

degree of feeling present when it happens not to be totally lost. Reflecting on the case, I ordered the mother to give the child three grains of calomel at once, and to follow it with the oil and turpentine early the next morning. I saw the child the following day, before the medicines had occasioned more than a single small stool, and carefully examining the legs observed no notable change. About seven hours later I found the boy on his mother's lap. As I entered he called my attention to his legs, saying he could move them, and, in fact, doing so. It seemed that the purges had acted freely, and that he had passed four worms and a good deal of slimy mucus. I was so astonished at this sudden relief that I was careful to see how far the improvement went. I found that he could stand with very slight aid. The following night he passed a worm, and within a week, under the use of worm tea, several others, while at the same time he continued to improve, until, at the close of a month, he was as well as usual, except, perhaps, a little limp in his gait and slight but continued ptosis. I set the case down as palsy from worms until March, 1855, when he had a series of convulsions, for which I could see no especial cause. They ended in a much more complete paralysis of both legs. I confidently repeated my former treatment, but, to my amazement, no worms came away at any time afterwards, yet a persistent purgative course so far improved the boy that in three weeks he could stand, holding fast to a chair. During the spring and summer he took iron and quinine, and was some time at the sea-side. These measures completed his recovery so far, that he seemed to me better than at any time since the first attack. Early in October, 1855, he had a repetition of the fits, followed by coma and resulting in paraplegia, complete as to motion and nearly so as to sensation, which latter, however, gradually improved, but without any like gain in movement. The means which at first had answered now failed entirely, unless the change in sensibility be regarded as evidence of good done by the purgation. As before, no worms escaped, although several anthelmintics were used in succession. Soon afterwards the family went to the West, and I lost sight of my patient. Three years later I learned that he had continued in the same condition as when last seen by me, until he had died

of dysentery, two and a half years from the date of his final attack of paralysis.

Was this a case of central disease from peripheral irritation of worms, or was it an instance of congestion of the centres relieved by the derivative agency of purgatives?

As regards paralysis, the result of dysentery, M. Jaccoud remarks that in many cases this latter malady is to be looked upon as a blood disease, productive of dyscrasial conditions, which alone may occasion defects of nutrition, and so give rise to spinal or cerebral disease. These remarks apply with much force to epidemic dysenteries. I myself have seen in the wards of the U. S. A. Hospital for Diseases of the Nervous System many cases of palsy, chiefly paraplegia, following upon dysentery, acute or chronic; but in nearly every case there had been many possible causes, such as long marches, bad diet, malaria or injuries to the spine—these being so common that almost any patient long in service had some such to relate. It was thus difficult, or even impossible, to fix upon any single factor as most important or essential, where, as in the mass of cases, it was likely that several contributed to influence the final result.

It is no new remark that diseases of exhausting discharges, such as diarrhæa, (and the same remark applies also to hyperpurgation,) may so profoundly deplete the system as to affect injuriously the nutrition of certain organs, and at least favor, if they do not produce, alterations sufficient to destroy function. It is for this reason that I hesitate to accept as a palsy, peripherally induced, every case which follows diarrhæa. In my experience at the hospital above mentioned these histories were by no means rare. In some of them the enteric malady occasioned spinal effusions or softening. In others the patient, worn out by long continued and constant evacuation, became paralyzed during a trying march, or upon exposure to great heat and fatigue. Such were, as I viewed them, cases of exhausted centres, and were so easily relieved by tonics, good diet, and stimulants, that we came at last to predict this result with a confidence not common as regarded other forms of paralysis. When reduced by depletory treatment, or when the patients were scrofulous or scorbutic, spinal softening or chronic myelitis was apt to occur, but even in these instances nothing

was more remarkable than the restorative value of wine, fresh meats, and bark.

As with diarrhœa, so with hyper-catharsis. A paralysis following either need not of necessity be due to transmitted irritation, since, in some cases these agencies so alter the blood, and so impair its nutrient value, as to give rise to serous effusions, or to still graver structural ills. Where there is a weak point, it is the one likely to be assailed, when by any means the circulation has become depleted beyond the point of endurance consistent with functional activity. The following case is an instance of such a result:

A young seamstress was for two or three years subject to terrible menstrual hemorrhages, from which she became excessively feeble and anæmic. At this time an apothecary told her that her liver was out of order, and advised a dose of calomel and jalap. I do not know how much she took, but it acted on her bowels with great violence, so much so that on the second day she took to her bed and sent for me. I found her still purging at intervals of an hour or two, and perfectly worn out. I ordered an injection of laudanum, and directed a drachm of paregoric to be given after each stool. Before I left she told me that her sight was very dim, that it had been weak for a long while, owing to incessant needle-work by gas light, and that since taking the purgative it was much worse. Upon this I examined her ankles, which I found to be slightly œdematous; as was determined that evening and afterwards, her urine contained no albumen.

The next day she was unable to distinguish faces, and complained of dizziness on trying to raise her head. She had no other notable symptoms in connection with the eye. Upon awaking from sleep the sight was always materially worse, but would so soon improve as to enable her within a few minutes to tell light from darkness, and to be aware if a person came between her and the light. One eye was always much better than the other, but I do not find that my notes record which it was. Although the purgation was at once checked, and she was put upon stimulants, iron, &c., no change for the better was noted until at least eleven days after the occurrence of the loss of vision. Recovery complete within three months, when she became able to use the needle in the day-time only.

Was this mere functional disorder, induced by the irritating influence of over-purgation? Was it sub-retinal effusion, or did it happen that the optic centres, exhausted by long and constant over-work, were the first to feel and the longest to show the effects of a great and sudden drain on the economy? One of the two latter explanations appears to me the correct one.

Paralysis following pneumonia or pleurisy is regarded by Dr. Brown-Séquard as functional, and explained under his theory of reflex impression on the spinal vessels. I have met with a number of such cases, but as all were in soldiers who had seen much service, I am not prepared from my own experience to admit reflected irritation from diseased lung or pleura as the only possible cause. Where great fatigues, extremes of climatic exposure, and bad or irregular diet have previously acted upon the victims of malaria, it is probable that the exhausting influence of an acute malady may with justice be regarded as the final determining cause of defective nutrition in the nerve centres. Such of these cases as I have seen were of a nature to induce this belief, merely from a study of their symptoms and mode of onset, which was sudden in very few of them.

While, however, I should explain many in this manner, I am quite willing to admit that, in others, the palsy has appeared to me to arise from an influence exerted on the spine by the irritating effects of a diseased viscus, and that in such instances there might have been, at least in the early stages of the neural malady, no distinct organic lesion of the central ganglia, of such a nature as to have been readily visible had a post mortem examination been possible.

As regards nervous diseases arising from dentition, there is less doubt in the minds of the profession in general, than in regard to most of the supposed causes of functional palsies. For my own part, I confess that it has very rarely been my lot to meet with cases in which the convulsions or the paralysis of childhood seemed to be distinctly traceable to irritation of the gums, arising out of the growth of the teeth. Indeed I might go still further and add, that of the many instances of convulsions and infantile paralysis which I have encountered, I do not recall

one case in which section of the gums at once occasioned a cessation of the symptoms in question.

It is curious to notice in this connection how much less cutting the gums is practiced on the Continent than in England or this country. Romberg specifically refers to this fact. Yet when we consider instances such as Brown-Séquard and others have related, where incising the gums did manifestly relieve the patient, or where the neural malady recurred anew as each tooth appeared,* I am unwilling to assert that it is always useless, though I am well convinced that, nine times out of ten, the gums are needlessly lanced, and that in severe neural maladies there are usually other and graver influences at work than those which arise out of the process of dental development.

M. Brown-Séquard has included among reflex paralyses, and therefore regards as due to peripheral irritation, the large class of palsies connected with eruptive fevers and diphtheria.

Dr. Gubler, who has made the most elaborate study of these paralyses, divides them into those which arise during the acuteness of the parent disease, and those which follow it at great or less distances of time. M. Jaccoud, in his remarkable work on paraplegia, briefly sums up these views, which I will still more briefly analyze here, as I have not before me the concluding portion of Gubler's essay.

The first class of paralyses is most common after typhus fever and variola, very rare in other exanthemata or in intermittents, and unknown as yet in diphtheria and in cholera. By general opinion they have been held to be due to organic disease of the centres; nor is it astonishing that this should be, when we reflect how marked are the early symptoms which, in the diseases first named, may be held to indicate effects upon the spine and brain. Nor, although rare, have autopsies been wanting to support this view, and to demonstrate the existence of early organic lesions amply sufficient to explain the precedent palsy, and of course referable to the direct action of the poisons peculiar to the several parent maladies.

As regards the secondary palsies, it is difficult to see how we can regard them as due to a peripheral irritation, or why

* Meryon on Paralysis, p. 178.

we should need to explain them as Brown-Séguard has done. Like M. Jaccoud, we feel "that hydrorachitis, cedematous infiltrations, and passive congestions, which result from enfeeblement of vascular contractibility, have been far too much lost sight of in the pathogenetic appreciation of these paraplegias."

We may add that the state of the blood which some of these maladies occasion is enough to account for any condition of the spine or brain which deficient or perverted nutrition may develop, and that these results will be ever most likely to obtain in constitutions predisposed by hereditation or other influence to scrofulous conditions.

Just such palsies have been seen to follow the intense blood poisoning from the bites of venemous reptiles, as Fontana had already asserted, and as I myself have once seen in man, and twice in dogs, who had been poisoned by the rattlesnake.

It will thus appear, from what we have witnessed, that it is in a high degree improbable that the palsies which occur during fevers and exanthemata, are due to a reflected irritation from the diseased surfaces, or from the viscera affected. Although the author so often quoted styles the palsies in question reflex, it is remarkable that he does not indicate the part or parts of the body from which the irritation may be presumed to proceed. From whence, for example, in typhus, do these impressions set out to affect the spinal vessels and so cause the loss of function?

Still more fully does this criticism apply to the cases which follow fevers, and, most of all, as M. Jaccoud observes, is it difficult to conceive of any such point of departure as regards diphtheria, whose sequelæ often follow the acute attack at very long intervals.

I should not have thought it worth while to dwell so long upon this subject if it were unimportant in practice, whether we regarded febrile and post-febrile palsies as functional or not. Many observers have shown, and Gubler most clearly, that the pathological conditions which are observed post mortem in these cases, are varied in their character, so that in some the palsy has been plainly due to œdema, or to mere congestion, with or without effusion, and in others to meningomyelitis of the gravest type. Any view, then, which would

group all these multifarious cases under one head, with a presumption that they are functional and involve no material lesion, would, we think with M. Jaccoud, be productive, if generally received, of the most serious errors in practice.

Among other supposed causes of disease of the centres, brought on by a peripheral influence, are the numerous instances of palsy which follow exposure to cold and moisture, all of which Dr. Brown-Séquard claims as reflex paralysis. In accordance with my treatment of other classes, I shall show, that in this one, also, it is not a reflected irritation from the skin which alone can be called on to explain the disease.

The records of cerebral or other palsies, from cold or exposure to wet and cold, are common in medicine since the days of Galen, but Dr. Graves seems to have first attempted to explain more precisely the mode in which he believed them to have been caused.

This class of cases presents two species. Those in which sudden palsy follows temporary exposure to cold, and those in which the palsy has been brought about in persons whose occupations subject them habitually to the inclemency of the weather. Many cases of the first species are on record. In some of them, there was plainly an apoplectic condition, in others, limited local palsies occurred. The best and clearest instances of the latter result were probably due to the peripheral irritation of cold.

The second set of cases were first referred to cold as a cause by Dr. Graves, who reported three histories of this nature. His first case, *Clinical Medicine*, p. 422, is too long to quote. It is in some points a very remarkable one. It began in Jan., 1829, with an attack of disordered stomach and bowels. At this date the patient abandoned the field sports which subjected him to the influence of bad weather. In March, 1832, having had many such paroxysms in the interval, he first felt some numbness and loss of power in the legs. In August, 1832, he became suddenly paraplegic, and died Sept., 1833. We observe that the palsy began three and a quarter years after he ceased to expose himself to cold and wet. With what reason can we claim that these agencies occasioned his palsy?

No lesions were found, but of course there was lacking a microscopic inspection.

Case second is described, as a waterman, frequently exposed to cold and wet, and in the habit of drinking freely. If we ask for a cause, here were two to choose from. Autopsy, cauda equina slightly softened, small tumor external to sheath of cord, some slight degree of vascular injection around it. Dr. Stokes, the reporter, considered these lesions incompetent to produce the paraplegia. No statement is made as to which columns of the cord were opposite to the tumor—a fact of moment in the decision, because sensation was merely lessened, motion destroyed.

Case third was a gentleman fond of field sports, and thus greatly exposed to wet and cold; while under the influence of a “long mercurial course” he got his feet wet, and numbness and weakness of the legs ensued. He recovered imperfectly. The history is clear enough, except that we do not learn why he was under the use of mercury.

Without wishing to be considered as hypercritical, I really do not see how the above cases can, in fairness, be regarded as of necessity due to the exposures described.

Paralysis from Wound or Injury of Nerves.—Within this class are to be found the most convincing instances of motor palsy, from a peripheral agency acting through nerves upon the central ganglia. In the best cases an injury is suddenly followed by paralysis in a remote limb; changes in the wound occasion increase or lessening of the palsy, and relief of the wound is very speedily followed by that of the motorial defect.

So far as I am aware, no case of motor palsy has been thus far reported which precisely comes up to this standard, although, in cases of spasms from peripheral irritation, every condition of the ideal case stated has been fulfilled.*

Perhaps the best histories are those in which palsy of ocular muscles has followed an injury to the infra or supra orbital nerve. Brown-Séquard, so industrious in his collating of cases, alludes to many where palsy followed upon dental disease, and

* Brown-Séquard, in Holmes' Syst. of Surgery. Diseases of Nerves, vol. iii., p. 878—cases given by Dr. Laing, Dr. Kimball, and others.

was cured soon after the removal of the teeth. He has also given numerous statements of neuralgia of the fifth nerve, which occasioned amaurosis, mydriasis, anæsthesia, &c.

Some of these instances are very convincing as to the peripheral causation. It is needless to add that in many of them elements of doubt exist, which have not the same application to paralysis from a wound or a blow on a part remote from that in which loss of function ensued.

Reports of this class of cases are very rare. In Brown-Séquard on Paraplegia none are given. In his lectures on the central nervous system he says: "Pressure on some sensitive nerve, or a wound, may cause extensive paralysis. So it was in a case that I have observed with my friend M. Charcot, and in cases recorded or mentioned by Fabricius Hildanus (quoted by Whytt, p. 18) and by Barthez, (*loco citat*, vol. ii., pp. 41, 42, *notes*, and p. 127,) 1852." I have already pointed out that the reference to Whytt is inaccurate, and that he reported no case of palsy. As regards M. Charcot's case, no particulars are given, which is much to be regretted, as it is now impossible to compare it with other cases. The reference to Barthez is incorrect, if Rilliet and Barthez' "*Sur les Maladies des Enfants*" be the book. In the work of these authors I can find no such cases, either at the pages mentioned or elsewhere.*

If, then, we except the cases of amaurosis from a blow or incised wound of a branch of the fifth nerve, we shall find histories of palsy occasioned by a wound or mechanical injury to a peripheral nerve extremely rare. Dr. Brown-Séquard reports one only in his paper on diseases of nerves in Holmes' *System of Surgery*, vol. iii. The case in question was due to a blow on the elbow, and was followed by paralysis of both arms, the uninjured limb being most affected. The length of time which elapsed between the injury and the palsy of the other arm is not stated, and the case is wanting in almost every necessary or usual detail. In quoting well known histories of disease such brevity is allowable, but surely not where the

* Dr Brown-Séquard, *Lect. on the Central Nervous System*, p. 164, quotes this book as Rilliet, and since he gives a reference elsewhere to Barthez—*Science de l'Homme*—the mistake may be here. The latter treatise has, I believe, but one volume.

author is reporting for the first time a case of great value and importance. Dr. Meryon, who favors the theory of reflex paralysis of Brown-Séquard, has related no instances of this nature caused by wounds.

Dr. Julius Althaus, in a recent little volume on neuralgia, etc., has reported three cases of paralysis following traumatic injury. In the first case, p. 155, the patient lost use of the arm after an amputation of the left forefinger for gangrene. When, after three months, the stump healed, the patient had entirely lost the use of her hand, the fingers being stiff and extended. The forearm could be bent with difficulty and pain. The stump was sensitive, and she was subject to a neuralgia which increased towards evening, and was referred to the lost member. These troubles were all readily relieved by faradization.

I do not feel sure that this was a palsy from peripheral disease acting on the centres. It is very likely that, as is usual, the hand was a long time at rest and on a splint, which alone, with the inflammation, would account for the stiffness and the rigid extension of the fingers, so rare in palsies of any kind.

But there was also neuralgia, with evidence that the cause existed far up the limb, so that there may have been a disease of the nerve, which, passing inwards, at length attacked the main trunks, and lessened motility in the member.

I should not have ventured to state that post-inflammatory stiffness and long use of splint might explain this case, if it had not chanced to me more than once to see, at the U. S. A. Hospital for Nervous Diseases, men who had seemingly lost power from nerve wounds, but who, on closer study, proved to have only that pseudo-paralysis which grows out of disuse and local disease.

In case second, p. 157, the history is incomplete, and is defective in clinical details. It is said to have been cured entirely, in two sittings, by faradization.

An ill set radius was rebroken and put straight, but the cure was protracted over ten months. At the close of this time, "the arm remained painful and entirely useless." Was this limb ten months out of use? Were no passive motions made in all this period, and was the arm kept at rest on a splint, or

not? Before deciding in favor of Dr. Althaus' diagnosis of the case as "reflex paralysis," we should like to be satisfied as to these points.

Case third, p. 158, is described as "reflex paralysis after rupture of capsular ligament." It is so utterly wanting in every thing which we may reasonably ask of a medical history, that it is vain to criticise it.

Before closing this part of our subject, I should point out one source of possible error in diagnosing paralysis or other disease as due to a wound, bruise, neuralgia, &c. The chances of mistake are perhaps confined to instances where an injury affects one nerve of a plexus, and where the resultant phenomenon (choreal, paralytic, neuralgic) follows at a later date and involves parts supplied by other nerves of the same plexus. In *some* of these cases it is probable that an inflammation of the nerve wounded may pass up its trunk, and involve one or more branches of the parent plexus. I think that I have seen this happen in several patients.* Whether such inflammations may travel backwards so far as to involve the ganglionic and central origins of nerves is a question which can not yet be answered, and which has indeed been scarcely thought of as a possible cause of disease. Needless to add that I do not refer to the passive nutritive changes which occur in nerves after section, but to active inflammatory or congestive conditions. The extent to which these latter affections may pass along a nerve is admirably illustrated in a case lately published by Dr. J. C. Nott, of Mobile, and which is, perhaps, in some respects, unique of its kind.†

* Wounds and other Injuries of Nerves—Drs. Mitchell, Morehouse, and Keen. Lippincott & Co. 1864.

† Bone and Nerve Surgery. Lippincott & Co. 1866. Philadelphia: pp. 91. In this extraordinary case neuralgia followed an amputation of the leg. To relieve him, three amputations were done. Finally, Dr. N. cut down in the popliteal space, exposed the ischiatic trunk and took out an inch of it and portions of the popliteal and peroneal nerves, altogether about three inches in length. These nerves were all enlarged. No relief followed. Next Dr. N. cut down on the stump, and dissected out its nerves. Still no relief. Amputation of the thigh came next with as little good resulting; and, finally, Dr. N. cut out an inch of the sciatic nerve below the pyriformis muscle. The nerve removed at this time was the first which had appeared sound, but it does not seem to have been microscopically examined. Partial relief followed. Curiously enough, the pain

Thus far I have sought to point out which of the cases usually explained as functional paralysis, or presumed to be caused by peripheral irritations might, with more reason, be assigned to other causes. Yet after every rational exclusion has been effected, we still have found numerous cases of palsy to which we can assign no possible cause, except an external irritation in some way acting through nerves upon nerve centres, to occasion loss of function. Especially is this true of the cases caused by mechanical injury, for here there are none or at least few of the difficulties which present themselves in connection with visceral disease. But even if a doubt still existed in our minds, it could hardly resist the evidence already offered elsewhere by us,* and which, in fuller shape, I shall presently re-state.

In one of the cases referred to a man is shot in the right neck; he falls with palsy of *both* arms. The ball has passed out; it has hit no large bone on which it could split, so as to make in its after-course a double injury, and thus account for the loss of power in the left limb. The right arm traumatically palsied, remains so for years; the left recovers spontaneously within a few weeks. Here the palsy is instantaneous, occupies a remote limb, and is to me inexplicable, unless I admit that the impression made by the wound of the right cervical plexus was transmitted inwards to the spine, and gave rise to loss of power in those parts of the medulla which give origin to the left cervical plexus. Other circumstances of this case, and the additional instances which I shall relate, strengthen this view—so strengthen it, indeed, as to leave little doubt in my mind that an injury of a nerve may give rise to sudden palsy of distant regions of the body.

I have almost as little doubt that an acute or chronic malady, or a sudden impression on the skin, may also give rise to paralysis; but the evidence in these cases does not place them on a level, as to credibility, with the palsies from mechanical injury; nor, in strictness, is it fair to infer from the latter, that if an injury may produce loss of power in remote parts, an acute

was in every case referred to the extremity of the existing stump, and not to the lost foot or leg. We should like to know the future history of this patient.

* Circular No. 6, 1864, Reflex Paralysis, Drs. Mitchell, Morehouse and Keen.

malady, as pleurisy, may do so. The cases are too unlike, and each must stand upon its own evidence.

Let us admit both classes, however, as I think we have reason to do, and what further explanation can we give of the mechanism of their production?

The older writers attempt none beyond what is involved in the term sympathetic, and even Whytt relates no case of palsy to be thus explained, although he uses his famous theory to account for so many other morbid phenomena.

Mr. Stanley* says, in substance: an irritation from a diseased organ is propagated by sentient nerves to the spinal cord, and through the motive and sentient spinal nerves to the limbs, occasioning in them loss of sensation and the power of motion.

Dr. Graves† thus explains himself: "The impression made by inflammatory derangement on the nervous filaments distributed to the mucous coat of the intestines is propagated to the spinal cord, and from this reacts on the muscular functions of the lower extremities."

Mr. Stanley is speaking of cases of palsy from genito-urinary disease, Dr. Graves of those arising from intestinal disorder. Neither explanation is satisfactory to the physiology of to-day, and neither goes practically beyond the step in advance made by Whytt,‡ when he conceived the idea that sympathetic morbid phenomena are effected through the agency of the nerves and the brain or spine.

Two theories remain. The first explains cases of palsy from peripheral irritation, by supposing that long continued or sudden and violent irritation of a centre through its afferent nerves may exhaust it so entirely as to occasion palsy of the external parts which depend upon it for their endowments. This view, less distinctly formularized, was no doubt held by many authors, and paralysis by irritation and consequent exhaustion sufficiently recognized. It was plainly set forth by Drs. Morehouse, Keen and myself, in March, 1864, and more elaborately, though not more distinctly, by Jaccoud, in December of the same year.

* Med.-Chir. Tr., vol. xviii.

† Clinical Lectures. p. 415.

‡ Works of Robt. Whytt, M.D., ed. 1768, p. 505 et. s:q.

Dr. Handfield Jones, 1861, and Eisenmann, 1860, are cited by Jaccoud as having ascribed some of the paralyses from abnormal peripheral influence, such as cold, to a shock (*un choc*) which abolished nerve power for a time. In this shape the theory would certainly rest upon no fair physiological basis, and would be open to the criticism which M. Jaccoud so liberally deals out while asserting his own claim.

If there be any great credit involved, the latter author was certainly anticipated by Dr. Gull, and also in the paper above referred to. Whether M. Jaccoud has fairly presented the claims of Jones and Eisenmann I can not say, as I have no access to their essays.

The remaining theory, that of Brown-Séquard, was first set forth in his lectures, printed in *The Lancet*, 1860, and more fully in his little volume on paraplegia, published in Philadelphia in 1861. His opinions were controverted by Dr. Gull in 1860, and by Drs. Morehouse, Keen and myself in 1864, as well as by M. Jaccoud. Nasse and Valentiner have also written against M. Brown-Séquard's theory, but I have been unable to obtain their papers. Romberg, at one time a believer in reflex paralysis, is said to have been influenced to a change of view by the criticisms of the authors last named.

I will endeavor to state, with all possible brevity, Dr. Brown-Séquard's theory, the evidence adduced in its favor, and the opponent criticism.

The fullest exposition of Brown-Séquard's ideas exists in the volume on paraplegia, by which, therefore, they may best be tested. He discusses at length the question as to whether there is a reflex paraplegia. With exceptions and much reservation as to cases and sets of cases, we have already admitted the existence of palsy from peripheral irritation, which, in other terms, is as far as his first proposition goes.

To comprehend the future argument, it must be remembered that under the term reflex paralysis, the author whose views are in question includes all the cases of palsy which have been supposed to proceed from acute disease, exanthems, dentition, worms, cold, injuries, &c.

The following paragraphs include all that Dr. Brown-Sé-

quard has here seen need to explain in regard to his theory of the mode in which peripheral irritations act.

They occasion, he says, a permanent spasm of the spinal vessels, and the want of blood and the insufficient nutrition which follows occasion palsy, from loss of function in the centres affected.

“ 1st. *Reflex Contraction of Blood-Vessels.*—As it is now well established that blood-vessels contract with energy, and sometimes even are seized with a real and prolonged spasm, whether by a direct influence of their motor nerves, or through an excitation, which, from some centripetal or excito-motor nerve, has been reflected upon them by the cerebro-spinal axis, there is no need of showing here that blood-vessels are just like muscles of animal life as regards their relations with the nervous system. This being the case, it is extremely easy to understand how a paralysis of the lower extremities, as well as that of any other part of the body, may be caused by a reflex action. In three different places a contraction of blood-vessels may cause paraplegia: 1, in the spinal cord; 2, in the motor nerves; 3, in muscles. A contraction of blood-vessels in the spinal cord I have seen (in the vessels of the pia mater) taking place under my eyes, when a tightened ligature was applied on the hilus of the kidney, irritating the renal nerves, or when a similar operation was performed on the blood-vessels, and nerves of the supra-renal capsules. Generally, in those cases, the contraction is much more evident on the side of the cord corresponding with the side of the irritated nerves, which fact is in harmony with another, and not rare one, observed first by Comhaire, (as regards the kidney,) and often seen by me after the extirpation of one kidney or one supra-renal capsule, *i. e.*, a paralysis of the corresponding lower limb. It is probable that irritations starting from the urinary and other organs, produce a paraplegia by a contraction rather of the blood-vessels of the spinal cord, than of those of the motor nerves and muscles. However, in this form of paraplegia, it is not rare that a notable diminution of temperature of the paralyzed limbs shows that the blood-vessels of these parts are also contracted.

“ 2d. *Morbid Reflex Influence on Nutrition.*—This influence, proved by many experiments on animals, and by pathological

facts of daily occurrence, seems usually not to exist in reflex paraplegia, except in that form in which muscles become progressively and rapidly atrophied and altered. (*Wasting palsy*.) But this morbid influence may take place at any moment in the course of a reflex paraplegia, so long as the irritating cause has not ceased to act, and a myelitis or some other affection may be generated by it.

"We think that it will now be considered possible, if not probable, that the production of reflex paraplegia is due to a contraction of blood-vessels, and to the insufficiency of nutrition that follows this condition of the vessels." (Lect. on Paraplegia, p. 23, Phil., 1861.)

Against the idea of spasm of vessels lasting for days, months, or years, every author who has criticised this view has in turn protested, as in essence singularly unphysiological.

Dr. Gull was, I think, the first to oppose this explanation of the etiology of palsy from external irritation. His paper, which is a model of medical criticism, we shall again refer to. M. Jaccoud has very fully treated the subject in a theoretical point of view, and, as his statement is inclusive of the former critic's opinion, I will resume it here. Certain additional arguments will be found in a quotation from *Med. Circ.*, No. 6, 1864, at another place in this paper.

To cause palsy, says M. Jaccoud, the contraction of the arteries must be permanent, or last as long as the palsy; but exhaustion follows abnormal excitement, and wherever vessels are concerned, passive dilatation succeeds to active contraction, and comes the sooner as the primary stimulation has been the more severe.

Thus the stern physiological law of rest after labor, of relaxation after contraction, stands in the way of any idea which presupposes long continued vasal spasm. M. Jaccoud thinks that the spine is so rich in vessels that it would not be possible by a local contraction of blood-vessels to cut off the vascular supply from any considerable section. I myself fancy that it would be hard to conceive of any spasm, such as would be competent to annihilate function, and yet occasion none of those visible defects of nutrition, such as are seen in the brain

when embolus occurs, or in rare cases after ligature of one of its main vessels.

As Brown-Séquard, a brilliant experimentalist, has appealed to observed facts, created purposely in animals, so his critics have replied by a like appeal to direct experiment. Paraplegia, says Brown-Séquard, may be brought about in animals by excision of the kidney or supra-renal capsules—a result which he himself has repeatedly obtained, in accordance with the much older experiments of Comhaire,* to whose observations all the writers refer briefly, without giving any further account of them. Raoul Le Roy d'Étiolles, experimenting with great care, has since failed of this result. Dr. Gull, aided by Dr. Pavy and Mr. Durham, repeated these experiments, taking great pains to avoid injuring the lumbar and psoas muscles. In no instance did paralysis of the hinder-limbs result, nor could it be told from the gait of the animal, after the operation, which kidney had been removed. Dr. Hammond† informs me that he himself was not less unfortunate, and in numerous ablations of the renal organs met with no instance of paraplegia.

M. Brown-Séquard, as quoted above, tells us that when he irritated the nerves of the kidney or supra-renal capsule, he saw the blood-vessels in the spinal pia mater contract, and that generally the contraction was most marked on the side upon which the nerves had been irritated. Now, it does not seem that the vessels remained in a state of spastic contraction, despite the violence of the irritant used. Nor, from what we know, should we expect such a result? In the meninges of the brain direct irritation is followed in succession by contraction and dilatation, while the same result obtains with no less clearness when, the sympathetic nerve being stimulated, the cerebral vessels first narrow and then enlarge, as Van der Beck Callenfels and Donders have shown.

On the faith of direct observation, Dr. Gull discredits Brown-Séquard's statement as to visible contraction of the spinal blood-vessels or irritation of renal nerves. He failed to get such a

* Comhaire, *Dissertation sur l'Extirpation des Reins*. Paris, 1803. I have been unable to obtain this essay.

† Dogs and rabbits were used by all of these experimenters.

result himself, and is, we think, rather too decided in his censure of the French physiologist.

In point of fact, neither party has given their experiments with the precision demanded in records of scientific investigations. As an instance, Dr. Gull does not say how long he waited after opening the spine before acting on the kidneys, nor does he tell us how much blood was lost. Yet these points are of the first moment, because it does often happen that after the spine is exposed some time elapses before its nerves are fully sensitive. As to the vessels, which, of course, must be arterial to have any influence, Dr. Brown-Séquard says he saw them contract, and Dr. Gull says there are none large enough to be visible. This latter statement may be affected by the conditions of the individual experiments; but since they are insufficiently reported, we must refer the dispute back again to the field of experimentation, where only it can be settled.

It is difficult, I think, to adhere to a theory which presents defects so obvious. In one respect only has it been unfairly dealt with by the critics. Brown-Séquard has loaded it with one condition which I believe to have been unnecessary, and which, as I have said, has been mercilessly handled. I refer now to the question of the existence of material lesion in cases of reflex palsy, or, as I prefer to call it, of palsy from peripheral irritations. Its importance, however, is not thus limited, for it extends to the whole class of so-called functional maladies, and whatever view we take of the palsies from external irritants must deal with this subject of immediate or ultimate material changes in the affected centres. Throughout his works, Dr. Brown-Séquard holds that paralysis from peripheral irritation exhibits no central lesion. If we exact of the autopsic examination that it shall have been aided by the microscope, we shall discover scarcely a case which will stand this test, if we except that related by Dr. Gull,* and claimed by Brown-Séquard as

* Guy's Hospital Reports. Third series. Vol. iv. Case 17, p. 174. As regards this patient, in whom paraplegia occurred, it does seem clear that there was no spinal lesion found by a very competent observer. The man died on the sixteenth day from the date of the paraplegic attack. There was but one omission at the autopsy. The head was not examined, nor, of course, was the medulla oblongata inspected.

a type case of reflex palsy. With more justice may it be regarded as a case of asthenia of the cord, which is what its reporter claims it to have been. Were we to admit that palsy may be occasioned by spasm of the spinal vessels, it is quite conceivable that if death should occur very early, no lesion might be found; but it does seem to me quite certain, that when there is prolonged want of blood in a part, material lesions must arise. For this case Brown-Séquard's theory in a measure provides, since he states distinctly enough that the palsy will be due to contraction of vessels and to the insufficient nutrition which this produces. Now, it is inconceivable that want of nutrition great enough to paralyze could long continue without originating lesions, which the microscope, at least, would detect. Nevertheless, we find Brown-Séquard insisting on absence of material lesion as one of the proofs of his cases having been due to spasm of the vessels.

I hold, myself, that where a continued or violent irritation has acted on the nerve centres through their afferent nerve channels, the palsy, occasionally induced, is due to loss of irritability in the ganglionic masses concerned, and I am not unwilling to apply the remarks I have just made to this theory, under which I should explain most of the cases of paralysis which Brown-Séquard terms reflex.

Material injury involves defect or loss of function. *Prolonged* loss of function, to my mind, involves also material change; and I firmly believe that the future history of this subject will bring us to this conclusion, so soon as a more exact study is made of the tissues of those who die after having been *for a length of time* the victims of paralysis from external irritation. If we conceive that spasm of the vessels occasions the result, it were easy to imagine the kind of lesions to be expected. If exhaustion from over-stimulation of the centres be received as the etiology of such cases, it is impossible, in the present state of our knowledge, to suspect what form of structural change would arise.

If, again, we conceive that external irritations may occasion loss of power through long continued paralytic dilatation of spinal vessels, as elsewhere pointed out, we should also expect to find material lesions in old cases; but whether there may

be in some recent ones congestion enough of this kind to occasion death, without being great enough to be visible post mortem, is a question which as yet has not been fully answered.

Further and more careful examinations of undoubted cases, recent or of long continuance, must be looked to for enlightening us as to the existence or non-existence of lesions. Meanwhile, it is encouraging that in epilepsy, insanity, and lately in tetanus, rigid scrutiny has detected alterations of tissue which will soon or late enable us to erase these maladies from the catalogue, now far too long, of merely functional disease.

Before concluding this, the critical portion of my essay, I will quote at length the objections to Brown-Séquard's theory, which were made by myself and my friends Drs. Morehouse and Keen, (May, 1864.) in the Army Medical Circular, No. 6, 1864. The language of this extract I feel no inclination to change. It states with sufficient fullness the views which I still hold in regard to the production of peripherally induced paralysis, whilst in many respects the arguments employed by us differ remarkably from any of those made use of in the able papers of Dr. Gull and M. Jaccoud.

"A gun-shot wound occurs, involving large nerves or not, and we have instantly a paralysis of motion and sensation, or of either alone, in some part of the body more or less remote. How shall we explain this? Although we have long been aware that certain forms of disease are capable of causing paralysis of distant organs, of altering secretions and affecting nutrition, we have had no plausible theory of the causation of these effects, until Dr. Brown-Séquard attempted to account for them in a manner equally simple and ingenious. Recalling the fact that irritation of the vaso-motor nerves is capable of producing contraction of the blood-vessels, he inferred that when an external nerve is violently or permanently excited, it may be able to produce contraction of the vessels of the nerve centres, and thus give rise to paralysis. It seems unlikely, even if we admit his explanation, that the arteries could remain contracted for any great length of time. But it is possible that the alteration of nutrition, which this temporary anæmia causes, may give rise to one of two results, either a continued disturbance of nutrition, which, however slight, would occasion

grave results if it existed in a nerve centre, or, secondly, to a paralysis of the vessels of the nerve centre involved.

"We suppose, first, the existence of an exterior nerve lesion; secondly, a consequent irritation of the vaso-motor nerves in a limited part of the spine; contraction of its vessels, anæmia, nutritive changes, and, finally, a relaxation of these tubes, which would be more apt to be a lasting condition, and would in fact constitute congestion. Such a series of consequences may very possibly occur, and would no doubt be competent to cause a paralysis, whose site, extent and character would depend upon the part of the nerve centres affected by the excitation. With so satisfactory an hypothesis before us in this modified shape, it would seem needless even to suggest any other explanation. But in a region of research so little explored, it may be allowable to point out the fact that another mode of explanation is at least possible, and the more so since there exist certain objections to Dr. Brown-Séquard's manner of viewing the subject.

"It is, to our minds, improbable that contraction of the vessels can continue for any great length of time. There is no experiment on record to show that this can be, or that it ever occurs in a nerve centre. We have therefore added the suggestion of consequent, and why may we not say primary paralysis of these channels? Here we have firmer ground for opinion, since it has been most distinctly shown that in section of the sympathetic nerve this result does take place, and is singularly persistent. But whether the blood-vessels remain contracted or dilated, nutritive changes would occur, and these the pathologist has failed to find.

"If, now, we ask ourselves the question, whether it may be possible to blight or exhaust utterly the power of a nerve centre, without the intervening mechanism of contracted or dilated blood-vessels, we are tempted to think that such a result may be producible.

"It appears to us possible that a very severe injury of a part may be competent so to exhaust the irritability of the nerve centres, as to give rise to loss of function, which might prove more or less permanent. A strong electric current is certainly able to cause such a result in a nerve trunk, while a general

electric shock, as a stroke of lightning, is, as we well know, quite competent to destroy the irritability of every excitable tissue in the economy. Now if the former of these results can occur in a nerve so insulated as practically to have no circulation, the loss of irritability can not be set down as due in such a case to a defect of circulation. Reflecting, then, upon the close correlation of the electrical and neural force, it does not seem improbable that a violent excitement of a nerve trunk, however brought about, should be able to exhaust completely the power of its connected nerve centre. The central change thus occasioned would, no doubt, involve the consequent or immediate occurrence of nutritive changes, which would gradually yield as time went on. While this view seems to us adequate to explain the facts, the notion of vaso-motor irritation and capillary contraction (Brown-Séquard) does not appear to be competent to cover *all* the facts.

"We have pointed out that no one has ever shown that vasal contraction can exist as a permanent state in a nerve centre. While, on the other hand, it has been proven that section of a sympathetic nerve involves permanent dilatation of blood-vessels; but in the brain, which is supplied by the sympathetic of the neck, division of this nerve gives rise to no disturbance, although the side of the brain on which the section occurs grows warmer. However, it is probable that the whole supply of vaso-motor nerves to the brain does not come from the neck, while other organs, whose whole supply we can cut off, as the kidneys, do certainly suffer nutritive changes as a consequence of such sections.

"One or other of the two theories we have offered must, therefore, be called on to explain the central changes which give rise to reflex paralysis. Either the shock of a wound destroys directly the vital power of a nerve centre, or it causes paralysis of the vaso-motor nerves of the centre, with consequent congestion and secondary alterations. *But there is no reason why, if shock be competent to destroy vitality in vaso-motor nerves or centres, it should be incompetent so to affect the centres of motion or sensation.*"

Before concluding, it is but just to call attention to a third view of the causation of palsies of peripheral origin. Dr.

Handfield Jones (*British Medical Journal*, 1859, Feb. 5th, and *Functional Nervous Diseases*, 1864,) proposed to use the term inhibitory, as applied to certain functional palsies. He is of opinion that the reflex palsies of Brown-Séquard and others are to be included in it. Dr. J. mentions the experiments of Pflüger, Lister, and the older observers, on the pneumogastric nerve. He agrees in part with Lister, modifying his views so that it is not merely the energetic operation of an afferent nerve that is supposed to cause inhibitory action, but its being injuriously affected by some impression made upon it. The enfeebled state of the nerve, or the nerve centre to which it proceeds, or the severity or malignity of the impression, may give rise to the peculiar effect.

Against this view, as explaining functional paralysis from peripheral irritation, it may be urged, that in health, inhibitory phenomena are only known to occur in the pneumogastric nerve, and within the range of the sympathetic; and hence that we have no right to stretch the case so as to cover pathological phenomena involving the spinal centres and nerves, or the voluntary muscles. That, finally, there is reason to suspect that the so-called inhibitory phenomena are due to exhaustion of nerves, or ganglia, by over-excitation. In fact, Dr. Jones admits that the paretic condition of the centre persists for an indefinite time after the removal of the morbid cause which affected it. In this respect only, he says, inhibitory palsy differs from reflex paralysis, which, according to M. Brown-Séquard's definition, increases, diminishes, or disappears as the morbid cause is greater or less, or absent.

It is plain, however, that this definition applies to but few cases, and that in his, as in Dr. Jones's cases, the paralysis often remained when the supposed cause had been taken away. Finally, Dr. Jones briefly urges against Brown-Séquard's theory of vaso-motor influence much the same arguments as those employed more fully by Dr. Gull, M. Jaccoud, and in our paper already referred to.

If I were now to sum up the probabilities in the way of causation of palsies peripherally induced, I should be disposed to refer some cases to exhaustion from too constant or excessive exercise of normal functions, and others to irritation

from disease or injury, and to consequent exhaustion of the centres; while, as regards the intervention of vascular agency, I should reject the idea of prolonged vasal spasm, and consider it possible that in some instances over-excitation might result in dilatation of the vessels, in which case some material lesion would surely result, if the condition in question were of long continuance.

On Tumor of Testis, containing "Fœtal Remains," with a Case.

By W. H. VAN BUREN, M.D., Prof. of Anatomy, University of New York, &c., &c.

A male child, two years and five months old, was brought to me in the month of October, 1864, with a tumor of the scrotum, apparently involving the left testicle, about the size of a large hen's egg. The child was healthy at birth, and the swelling of the testis was not noticed until he was three months old, when a physician was consulted, who, regarding the disease as a hydrocele, passed a seton through the tumor, in the form of a worsted thread. So much pain followed this operation that the seton was withdrawn in three hours, a glairy fluid escaping with some force, but in small quantity, and the tumor remaining hard, and subsequently growing harder under the very severe inflammation which followed. After the subsidence of the inflammation the tumor remained about at its original size, but somewhat harder, for nearly a year, when several abscesses formed and discharged themselves consecutively; and finally, after quite a large abscess had opened, a red, fungous mass protruded from its orifice, which gradually reached the size of an English walnut.

I found the tumor presenting fully one-half of its mass in the shape of this fungous protrusion, which was covered with unhealthy granulations discharging watery pus, hard to the touch, and occupying its lower and larger portions. Its upper portion, towards the spermatic cord, was generally smooth upon its surface, of a soft solid consistence, the skin adherent, and not very painful when grasped. It had never been painful except when the abscesses were forming, and when it was punctured. There

were no enlarged glands in the groins. The child was weakly and pallid, and suffering from diarrhæa.

Dr. Valentine Mott had seen the case, suspected that the disease was malignant in its nature, and advised its removal.

I suggested to the father that he should take the child to the sea-side for a month, and pay close attention to his diet. At the end of this time his health was very much improved, but the tumor was unchanged. I was able to form no positive opinion as to its nature, but felt no doubt as to the propriety of its removal, and I therefore removed it, by castration, in the usual manner.

The child made a rapid recovery after the operation, and I have heard, within a few days, that he is in excellent health and growing finely.

On examination of the tumor, after removal, the portion already described as a fungous protrusion, and which occupied the lower part of the tumor, was found partially covered at its lowermost surface by integument, and upon this integument, posteriorly, was a surface half an inch in diameter thickly covered with hairs, some of them an inch in length, and presenting, under the microscope, the characteristic appearance of hairs from the head. Upon the surface of the protrusion was the orifice of a fistula, and on introducing a probe into the fistulous tract it came in contact with a very hard, smooth, apparently bony, surface. When laid open by the scalpel, a cavity was disclosed about an inch in diameter, containing pus, and in contact with, and adherent to its walls, a fragment of bone, covered by periosteum resembling in shape a fragment of the body of a foetal lower jaw-bone. The length of this fragment of bone was five-eighths of an inch, its breadth three-eighths of an inch, and its thickness about the same. Implanted somewhat irregularly upon one of its margins were found four teeth, slightly altered from their normal shape, but distinctly recognizable as two incisors, one canine, and one molar, in their normal relation, and appropriate in size to that of the fragment (of jaw-bone) in which they were implanted. On withdrawing the molar tooth from its alveolar socket, which was normal in its proportions, it was found to present the crown of a well formed molar, hollow within, and

destitute of fangs. At the bottom of the alveolar socket the dental sac was distinctly visible, and protruding from its floor was the well formed surface of a second (permanent) molar tooth, which, when touched by a probe, was soft and evidently not yet encrusted by enamel. It was with one of these teeth that the probe came in contact when introduced into the fistulous tract. Both the tooth and bone structure were examined microscopically, the former showing enamel prisms, and the latter the lacunæ and canaliculi of true bone.

Situated above the cavity, which inclosed the bone and teeth, was a second cavity containing turbid fluid, and lined by a smooth, apparently serous, membrane—the probable remains of the tunica vaginalis. In contact externally with the walls of this cyst was the testicle, normal in size and appearance, with the exception of an abscess, the size of a large pea, situated in its substance. The glandular elements of the testicle were recognized under the microscope. The mass of the tumor situated above and around the testicle, and constituting about one-half the tumor's bulk, was found under the microscope to consist of the elements of connective tissue, consolidated by inflammation.

The microscopic examination of the specimen was made by Dr. J. W. J. Gouley.

It is evident that the tumor is an example of that rare pathological condition known heretofore by the English as "fœtal remains in the testicle," and by the French as "*inclusion scrotale et testiculaire*." The latter designation is the more intelligible of the two, as it indicates the nature of this curious growth, which is an imperfect effort at the production of a double monster, or "monstrosity by inclusion," or *fœtus in fœtu* of the older writers. It is described as one of the varieties under this latter head in Vrolik's classification of fœtal deformities, and he remarks that "it is most probable that the *fœtus in fœtu* is an incomplete effort to form a double monster." (Cyclopædia of Anatomy and Physiology, art. Teratology.) The term "monstrosity by inclusion" belongs to the great French teratologist, Geoffroy St. Hilaire, and has been adopted by Cruveilhier in his "Pathologie Générale," and applied to tumors containing fœtal remains which have occurred in different parts of the body, *e. g.*, in the perineum, over the sacrum, in

the thoracic and abdominal cavities, the liver and ovaries, as well as in the scrotum.

This explanation of the pathological nature of these tumors has been more recently disputed by Lebert, who endeavors to include them in his class of "dermoid cysts," or misplaced growths of normal tissues. The pathological law under which all these growths are developed is thus stated by Lebert, (*Traité d'Anat. Path.*, t. i., p. 260.) "That both simple and compound tissues, and even more complicated organs, are capable of developing themselves in parts of the body where normally they do not exist." This he considers that he has established, and its truth is generally admitted by pathologists. But Lebert does not entirely exclude the theory of inclusion, as will be inferred from the following quotation: "I have brought together three cases of dermoid cysts of the scrotum, and endeavored to establish the points of difference which distinguish them from true cases of foetal inclusion occurring in this same locality—in which undoubted debris of the skeleton are recognizable." (*V. ut supra*, p. 257.)

The theory of inclusion of St. Hilaire has also been disputed by the latest authority on the subject, Dr. George Murray Humphry, lecturer on surgery and anatomy in the Cambridge University Medical School, the author of the article on "Diseases of the Male Organs" in Holmes' *System of Surgery*, who considers that Lebert's theory of "heterotopie plastique" is entirely sufficient to explain the nature of tumors connected with the testicle containing foetal remains. (*Holmes' Surgery*, v. iv., p. 600.)

The question as to the real origin of these tumors appears, therefore, to be still an open one, and it may be stated succinctly as follows: Is a scrotal tumor, containing so-called "foetal debris," the result of a local plastic effort determined by injury or inflammation, and liable to occur at any period of life; or is it the production of a fecundated Græffian vesicle accidentally included in the scrotum of a twin foetus, and thus arrested in its development, and of necessity congenital? This question is more curious than practically useful, for, as Dr. Humphry concludes, the only remedy for these tumors is to remove them by operation. Those desirous of pursuing

it further will find it elaborately discussed by Lebert, (as above,) by Cruveilhier in his *Pathologie Générale*, t. i., p. 370, and t. iii., p. 582 et seq., and by Verneuil in the paper referred to below.

It follows, if Cruveilhier is right, that tumors connected with the testicle of this character must be always congenital, and such appears to be the fact. M. Verneuil has collected all the authentic cases on record, to the number of ten in all, and treated the subject very ably and exhaustively in a series of papers published in the *Archives Générale de Médecine* in 1855. The earliest recorded of these cases is the only one of the ten in which the congenital character of the tumor is not clearly demonstrated. "A young man of quality, after exposure to sexual excitement, was seized with a sudden pain in the right testicle; this soon subsided, but shortly afterwards he discovered an unnatural growth connected with the testis, which rapidly increased to the size of the head of an infant of six months, and within the year was removed by a surgeon of Sisteron, in France, named St. Donat. On opening the tumor, after its removal, it was found to contain the somewhat altered remains of a foetal cranium; the testis was compressed and altered in appearance, and the foetal remains seem to have been enclosed in a cyst attached externally to the testis." The case was transmitted by St. Donat to Pierre Amand, a member of the Faculty of Paris, and published by Amand in a volume on *Obstetrics*, at Paris, in 1715.

In a case reported by Prochask, "an otherwise well formed male infant was born with a small tumor in the groin, which was taken for a hernia. When three years old it commenced to grow, rapidly filled the scrotum, and in a few weeks reached as low as the middle of the thigh, when an abscess formed and discharged a fetid fluid, together with several portions of the skeleton of a foetus, after which the child rapidly got well."

The following case, reported by Ollivier, (D'Angers,) presents some features similar to mine: "Ovide-Emile Caze, well formed at birth, was discovered by his parents, when a year old, to have the right testicle larger than the left, and six months later was operated upon by Dr. Capon, for hydrocele. A little serous fluid followed the puncture, but the testicle re-

mained larger than before, so that two years afterwards another operation was talked of, but as the swelling was painless nothing was done. During his seventh year, the testis, having reached three times its natural size, became painful, and an ulceration having taken place, a reddish mass protruded, in which Dr. André having discovered a hard, white, polished surface resembling a tooth, diagnosticated a tumor connected with the testis, containing foetal remains. The protrusion increasing it was tied off, and afterwards examined by Ollivier, and found to contain four teeth and a piece of spongy bone, contained, apparently, in a sort of cyst. The child, who was left mainly to nature, was thought likely to get entirely well. (*Mémoires sur la Monstruosité par Inclusion; Archives Générales de Médecine*, t. xv., p. 540.)

In Velpeau's celebrated case, which occurred in La Charité Hospital, in Paris, whilst I was an externe in that institution, in 1840, the patient, who was 27 years of age, had a tumor the size of the fist on the right side of his scrotum, which had existed since his birth. It was painless, and presented several fistulous openings, from one of which a tuft of hair projected, and this circumstance suggested the true nature of the tumor. Velpeau made it a point of saving the testicle, which could be distinguished from the tumor, although closely connected with it, and this necessitated a long and difficult dissection. The tumor contained much foetal debris and a number of easily recognizable bones of the foetal skeleton. The patient died of purulent infection. The case is recorded in the *Gazette Médicale de Paris*, Feb. 15th, 1840.

In M. Verneuil's case, which occurred in the wards of M. Guersant, in the Children's Hospital of Paris, the foetal debris were very carefully examined by the microscope, and, amongst other tissues, the histological elements of the gray substance of the brain were distinctly recognized.

Of the ten cases collected by M. Verneuil but two were diagnosticated; those of André and Velpeau. If the congenital character of these tumors is admitted, it constitutes their most valuable diagnostic feature. The diagnosis would lie between hernia, hydrocele, encephaloid cancer and tubercular disease of the testis. It would seem easy to exclude the two

former, although two of the cases noted in this paper were mistaken for hydroceles. Robert speaks of a case of congenital soft cancer of the testis; and I once saw a well marked case of syphilitic enlargement of the gland, in a child of eighteen months, who also had periosteal swellings and other evidences of inherited disease.

It is not unlikely that there are other cases of this curious affection which have not yet been placed on record, and if this imperfect notice of the subject should lead to any further additions to our knowledge by eliciting unrecorded cases, or by rendering their nature more apparent, it will have attained its object.

NEW YORK, Jan. 15th, 1866.

On Simulated Amaurosis. By Dr. C. SCHWEIGGER, Professor at the University of Berlin.

[Read before the New York Ophthalmological Society.]

The detection of simulated amaurosis, by involving the individual in contradictions with the simplest laws of physiology, and thereby compelling him to speak the truth, while he imagines himself to be giving support to his false assertion, affords an interesting illustration of the exactness of ophthalmoscopic science.

The means we have on hand for this purpose are very simple, but of course differ somewhat according to the alleged morbid symptoms. Mostly it is monocular amblyopia or amaurosis which is simulated, very seldom binocular, and only the more experienced and skillful simulators choose to pretend a moderate degree of binocular amblyopia, or a narrowing of the field of vision. With men we are generally able to find a more or less obvious reason for the simulation; not so with women, however, who often, to gratify some inexplicable whim, resign for years and years every sort of social enjoyment only for the sake of pretending a non-existing disease.

The suspicion of a simulated amaurosis is raised whenever we find a high degree of amblyopia, together with an iris actively contractile in dim illumination, and a perfectly normal

state of the optic disk and the retina, shown by the ophthalmoscope. It is important to know that a perfectly normal state of the ophthalmoscopic image may be combined with amaurosis or amblyopia of both eyes, not only, for example, in cases of alcoholism or uræmia, but also without any other perceptible disease; but in the latter cases there is either a speedy recovery of vision, or the fundamental disease of the optic nerve becomes visible by the ophthalmoscope.

Simulated amblyopia of a medium degree is best detected by the use of Snellen's test types, by an accurate determination of the degree of amblyopia, and by repeating the experiment with letters of different size, and at different distances. We have, for instance, a diminution of vision of $\frac{1}{20}$, then No. 200 ought to be seen at 10', No. 100 at 5', No. 75 at $3\frac{1}{4}'$, No. 50 at $2\frac{1}{2}'$, and so on as far as No. 10 at 6 inches. It will be a pretty difficult task for a simulator not to entrap himself in contradictions, if care be taken to measure accurately the distances at which the different types are made out, and to note down every single statement. If we have to deal with a narrowing of the field of vision, the best plan will be to sketch the letter accurately on a large sheet of paper. In order to keep the same distance for all designs, I use a rod of about 12 inches in length, which the patient himself fixes with his head against the paper, so as to make it impossible to alter the position of the head in such a manner as to render the projection of the field of vision larger or smaller. On repeating this experiment carefully several times in the course of some days, and on comparing the different projections, we shall soon discover a marked discrepancy between the patient's separate statements in cases of simulation. If the field of vision is alleged to be contracted in one direction near the point of fixation, we have a very elegant method of testing at once the truth of the assertion. After having drawn the projection of the visual field, we leave the patient in the same position, but with both eyes open. Now if we hold a prism before one of his eyes, double vision is the natural consequence; and, holding the base of the prism in the right direction, we may approximate one of the doubly seen points of fixation to the limit of the alleged field of vision. Adding then a stronger prism in the same direction,

we can bring one of these double images beyond the alleged limits, when double vision must cease, and only one point can be seen if the contraction of the field of vision really exists—double sight being of course impossible if one of the double images is formed on an insensitive part of the retina. The degree of the prism necessary to make the experiment conclusive may be found, by ascertaining the linear distance of double images produced by our prisms in the same distance we use for projecting the field of vision. After having ascertained this, we may easily arrange matters so as to make it entirely impossible for a simulator to know when he must see single and when double. One thing more ought to be taken into account, namely, the fact that double images produced by prisms with their base outwards may be united, and single vision procured by a corresponding convergency of the visual axes; by divergency, prisms of medium strength with their base inwards can be neutralized, while only the weakest with their bases above or below can be overpowered by a contraction of the inferior and superior recti muscles.

The detection of simulated amaurosis of one eye by prisms was first taught by v. Graefe. After having examined the alleged amblyopia, we carefully try the sight of the other eye, and holding a prism in front of it with the base upwards or downwards, inquire whether a small point or a fine line appears simple or double. Most of the simulators, imagining that we are dealing merely with the sound eye, at once acknowledge double vision, and in this manner I have several times made them read the smallest print with the alleged amaurotic eye alone, covering the sound eye by-and-by without their knowing it. In some cases, however, I tried the prisms without effect; either the simulators were already aware of their use and would not acknowledge the double images, or there was indeed no simultaneous vision with both eyes. Thus we see, for instance, exceptionally in cases of squinting, that each eye separately sees well enough, while both eyes never work simultaneously, so as to make it entirely impossible to bring on double vision by prisms. It is also possible, though not very probable, that similar cases may occur without squinting. In cases, therefore, where there is nothing to be done by prisms, I

tried to procure other means to detect simulation, and found them in the use of the stereoscope. In this instrument each eye has a separate field of vision, quite inaccessible to the other eye. This separation is the essential point of the instrument; convex lenses are added only for the purpose of approximating the stereoscopical designs, allowing at the same time a relaxation of accommodation and nearly parallel lines of vision; in myopics, therefore the convex lenses might be dispensed with. Prisms with their base outwards, combined with the convex lenses, have only the effect of enlarging the outer side of both fields of vision. For all the purposes we now speak of, this combination of prisms with convex lenses is quite superfluous, even, in some respects, troublesome.

Knowing, now, that in simultaneous vision, both separate fields of the stereoscope are united in one, simulation becomes evident if we find this union in a case of alleged monocular amaurosis. We have, for instance, in the one field, horizontal parallel lines about one-quarter of an inch apart; in the other, vertical parallel lines, separated by the same distance; then, in the united field of vision, we see both systems of lines united in regular squares. As soon as these squares are seen, therefore, it is evident that there can be no monocular amaurosis. We may even go a step further, and use the stereoscope quite independently of the existence or non-existence of simultaneous vision, and, in this respect, it is superior to the prisms. If we draw in each separate field of vision that vertical line whose image goes through the centre of the retina, then, in the united stereoscopical field, not only both lines are seen as one, but every object situated to the right of one of those lines is projected to the right side of the field of vision, and appears as if it were seen with the right eye. The same, of course, is the case with the left side. This gives the means of determining, in cases of simulated monocular amblyopia, the acuteness of vision, and, if we choose, even the range of accommodation. For this purpose we arrange matters as follows: We have at the bottom of the stereoscope a sheet of paper, marked only with the two lines above mentioned. Now if we have to deal, for instance, with an alleged amblyopia of the left eye, we place in the left field of the stereoscope, but to the right side of the vertical line,

any object, say a piece of a printed paper—with this exception, the whole of the bottom of the stereoscope is left blank. In the united stereoscopical field the paper will then appear as the right side, and will make so strong an impression that it is seen with the right eye, that I doubt whether anybody can resist it; with a stereoscope which allows the convex lenses to be approached to or withdrawn from the bottom of the stereoscope, we can, if we choose, at the same time, ascertain the range of accommodation.

I take this opportunity of showing you another interesting stereoscopical phenomenon, namely, Prof. Dove's method of detecting counterfeit paper-money by means of the stereoscope. On uniting, stereoscopically, a genuine note with its counterfeit, we see a very peculiar appearance of the small print or the system of fine lines with which those papers are usually adorned. The united stereoscopical images seem not to be situated on the same level, but offer striking irregularities. Some of the small lines, letters or words, are elevated above the level of the corresponding ones; others are drawn backwards. Not having been able to procure a bad note, I prefer demonstrating the phenomenon in another way. If we unite, stereoscopically, two copies printed with the same set of types, the whole of the stereoscopic image is seen in the same level. But if the copies are not printed with the same set of types, as is the case if we take two different editions of the same book, then we see clearly the difference alluded to, in the level of different letters or words. It is necessary, however, to look through the stereoscope for some time, in order to see the phenomenon in all its distinctness. The following remarks may serve as an explanation.

In two papers printed with the same set of types, the distance between one letter and any other one is the same in both fields of vision. After having, therefore, united two identical letters, we use the same relative position of the axis of vision to unite any other indetical letters. This we can not do if we have to deal with two different sets of types, because in the latter case the relative distance from one letter to any other is not the same in both fields of vision. Therefore, we have to

use different relative positions of the axis of vision to unite the identical letters, and this necessary change in the position of the axis of vision accounts for the apparent differences of the level at which the lines, words and letters appear.

PROCEEDINGS OF SOCIETIES.

NEW YORK PATHOLOGICAL SOCIETY.

Stated Meeting, October 11, 1865.

Dr. GURDON BUCK, President, in the Chair.

THE DEATH OF DR. D. S. CONANT.

The President announced the death of Dr. CONANT, a former President of the Society, and remarked that it had heretofore been the custom, under such circumstances, of taking some official notice of the sad occurrence by the passage of suitable resolutions.

On motion, the following gentlemen were duly appointed to prepare such resolutions, and present the same for adoption at the next meeting of the society: Drs. Markoe, Bradley and Eliot.

Dr. BIBBINS, having been invited so to do, then proceeded to give, as far as he was able, an account of the last illness of Dr. Conant. A week ago last Sunday, said he, one week before he died, I noticed, on his returning from the afternoon service at church, on the right side of the bridge of his nose, what appeared to be cellulitis, which was progressing. The part had been opened, I think, that day, and I heard no more of him until Wednesday evening. On returning home late that evening, I found a message on my slate, requesting me to see him at once. On going to his house I found him very ill, and in bed. The disease had progressed to a considerable degree, and he had marked typhoid symptoms, although he was perfectly rational. I saw him again on Thursday evening—his pulse was then 108, and he was suffering more from depression than the evening before. As he had a number of medical gentlemen at that time in attendance, I did not offer any professional assistance. On Friday morning Dr. Parker was met coming out of the house, and said that Dr. Conant's case was a very grave one, and that he thought that the prognosis was exceedingly unfavorable.

Dr. BRADLEY remarked that the pustule was opened on Monday evening by Dr. Peaslee, who stated at the time that there was no discharge from it to any extent, save blood, about four ounces of which escaped. After the bleeding stopped, the part appeared to dry down. The inflammation after this seemed to follow up the nasal bones towards the orbit. On Monday I saw him first; his right eye was somewhat swollen. He was attacked at that time with pleurodynia, which Dr. Peaslee considered as indicating the absorption of matter. Those pains were relieved somewhat by the application of mustard to the spine, and other rubefacients. Dr. Peaslee did not like to give the anodynes, for fear of disturbing the capillary circulation. Dr. Conant, however, had taken some morphine on his own account, before Dr. Peaslee saw him. On Tuesday the eye was very much distended. On Friday night Dr. Parker made some free incisions into the lids of both eyes. The wounds bled freely, but no pus was discharged. The conjunctiva were everted and presented the appearance of a diphtheritic membrane. The disease on Sunday attacked the left eye, and he was not much delirious until Sunday morning. At that time the sight of the right eye was entirely destroyed. The lids were then very much swollen. About eleven o'clock he began to be delirious, and continued in that condition until a quarter of eight, when he died.

Dr. SANDS then gave the following account of the post mortem examination, presenting at the same time the brain of the deceased for inspection. He made the examination with the assistance of Dr. Ball, and it was commenced with the impression that it would be impossible to proceed with any investigation of the brain, as it was said that by the process of embalming a sharp instrument had been thrust into the organ through the nostril, and injections had been thrown into the cranial cavity. The examination then was made with a view simply of inspecting the parts within the orbit. A section of the scalp was made, and it was turned down over the eyes. The cellular tissue of the orbit was spongy and infiltrated chiefly with blood. The blade of a scalpel was passed deep into the orbit, and a small amount of pus mixed with blood escaped. The eyes were not examined, as it was desirable not to disfigure the face. Dr. Sands felt very curious to know whether the inflammatory action found in the orbit had extended backward into the cranium, and, at a venture, a section of the calvarium was made. On cutting through the dura mater covering the anterior lobe of the right cerebral hemisphere, abundant traces of severe inflammation were there found in the shape of a thin layer of pus over the inner surface of that membrane. The upper surface of

the anterior lobe of the cerebrum of that side was bathed in pus and lymph—chiefly pus—which presented externally and beneath the arachnoid membrane. This part of the brain presented an unusual degree of palor. This effusion was traced over that side of the brain to the under surface of the organ, where it was found equally abundant. Pus was not only present on the anterior lobe, but also in the fissure of Sylvius. There was little or no matter beyond the middle lobe. The cranial bones corresponding with those parts of the cerebrum were found also diseased. There was a patch of lymph covering the lesser end of the sphenoid, as it forms the roof of the orbit, and was about as thick as oil silk or thick paper. There was also in this situation a small quantity of pus. In the middle fossa of the skull the purulent exudation was very abundant, and it was present not only upon the inner surface of the dura mater, but between the dura mater and bone. An examination was then made in order to ascertain through what channel the inflammation extended into the cranium. The spongy tissue on the right side of the sphenoid, when cut into, was found infiltrated with pus, just as was the case with the cellular tissue in the right orbit, showing that the diseased action had extended through the sphenoidal fissure into the cranial cavity. The optic nerve presented nothing abnormal. The spongy tissue on the left side of the sphenoid bone also presented traces of the inflammatory action in the shape of pus. In addition to these purulent deposits, there were scattered over the brain various ecchymotic spots. The results of the post mortem examination proved that death was caused by inflammation of the meninges of the brain. The brain weighed three pounds and three ounces after its removal, and it was fair to presume that at least three ounces of fluid had escaped before. The periosteum covering the frontal bone in the neighborhood of the right orbit was elevated, but not detached. There were no traces of inflammation of the veins about the face.

Dr. PARKER next gave the following account of the case. My first knowledge of Dr. Conant's case was on Thursday morning, when his sister called at my office with the request that I should see him, as he was supposed to be suffering from the so-called malignant pustule. I made an appointment to meet Dr. Peaslee in consultation at 1 o'clock. I obtained the following history of the patient. He had lectured on Friday, and on the day following he noticed a pustule on the side of his nose. He consulted Dr. Peaslee the same day, who, on examination of the part, concluded to open it. An incision was made, but whether with a thumb lancet or not I do not know; and whether it

was made directly down to bone or not, I am not positive. I found the patient under the influence of morphine, which had been taken to alleviate the pain which existed about the orbit. His pulse was 96 and rather quick, and his respiration was about normal. He had taken some medicine and his bowels had been relieved. His kidneys were acting well enough, his skin was warm, and hands a little dry; his tongue was also dry, which was ascribed to the anodynes he had taken the night previous.

The swelling had passed from the point of beginning on the right side of the nose to the right upper and lower eyelid; the lids were a little tumefied, yet they could be easily raised; in fact, he was able to open the eye himself.

There was tenderness just above the orbit near the inner canthus and in the neighborhood of the supra-orbital nerve. There was also some tenderness on pressure in that situation. There was some pain present, but it was not oppressive in character; there was no heat, no disturbance of the intellect, nor rigors. The room was ordered to be kept well ventilated, and all intercourse with friends strictly interdicted. He was taking stimulants, and the quinine, which he had also been taking on account of the stimulating effect which it produced, was changed for cinchona bark.

We arranged to meet the next day, on Friday, at about the same hour. I sent up my man in the evening, not being able to go myself, to inquire how the Doctor was, and the answer returned was that he was about the same. The next day we met, at one o'clock, and we found that he had a fair night's rest and that he had slept quietly. His pain was less that day than it had been the day before; his skin was moist, and pulse 86. His tongue also was moist, and the thirst which he had suffered had abated. There seemed, on the whole, to be some improvement in his general symptoms; although there was then some tumefaction of the right eye. There was no exophthalmus, and vision was perfect. At three o'clock, on Saturday, I saw him again and found his symptoms all aggravated; he was in more pain, his pulse had become more rapid and frequent, his skin was warm, and the tumefaction about the eye was very great indeed, involving the whole orbit—the eye itself protruded very perceptibly and the conjunctiva gushed out between the palpebræ. Vision of that eye was then entirely gone. The swelling extended nearly to the roots of the hair on the right side of the forehead, and over on the temples, the whole pitting on pressure. An examination of the face showed none of those appearances which are referred to an affection of the

veins. No symptoms of inflammation within the cranium showed themselves, excepting perhaps that there was a little more pain than before, and that he had had a little delirium for a short time, about four o'clock that morning, which manifested itself in a desire to look after some of his patients. His pulse in the afternoon was 130; his breathing had markedly changed, and had become irregular in character and was accompanied with a moan. I tried to see Dr. Peaslee and confer with him, but could not succeed in finding him, and called again in the evening, after dinner, when I was more fortunate. We went together to see him, and found him asleep. There was no incoherence, some pain in the head, and the pulse was 140. The respiration was rapid and moaning, and the tumefaction had extended, but not sufficiently to interfere with the other eye. The question was, whether there was any matter in the right eye which had destroyed that organ, and we decided to explore it, and three openings were made into its upper surface, and two into its lower portion. No pus escaped as the result of the incisions. This was on Saturday evening about ten o'clock.

At quarter before nine on Sunday morning we saw him again, and found that the case had advanced very rapidly in the wrong direction; the left eye was very much swollen, and it was then very clear that there was but one event to look for, and that was death. He died in the evening. We did not look upon the case as a serious one until Thursday.

Dr. STEELE, in answer to a question from Dr. Bibbins, remarked that the weight of the brain could not be perceptibly increased by the injection. He further remarked that he believed that what was mistaken for pus was really the injection, and the red appearance of the membranes and brain was also to be explained on that supposition.

Dr. SANDS stated that he recognized the injection very distinctly, but the appearances of pus were to him unmistakable, and furthermore the matter had been examined microscopically by Mr. Wynkoop, who substantiated the assertion.

Dr. STEELE remarked that he had thrown the injection into the membranes.

Dr. SANDS stated that the membranes, on post mortem examination, were found uninjured, and were dry, except in those portions where the creamy substance, which he called pus, was found.

Dr. DRAPER, who was present at the autopsy, confirmed Dr. Sands' statement with reference to the existence of purulent effusion. It presented precisely the same character which he had often seen in that

situation, and it did not occur to him how it could be any thing else. It was upon the meninges, and between the dura mater and cranium.

Dr. BRICK suggested that, if the substance were really injection, it could scarcely be found lodging outside the dura mater, when it had been introduced by way of the nose.

Dr. SANDS did not see how a sharp instrument could be passed through the nostril into the brain, without showing some evidence of its passage on post mortem examination. There were no evidences whatever of an instrument having entered; the ethmoid bone was intact, the cribriform plate was entire, and the base of the brain was uninjured.

Dr. BRADLEY, who was present during the embalming process, said that he saw Dr. Steele thrust the instrument through the nostrils, and that it passed up in the direction of the brain, evidently through bone, to a distance of at least four or five inches. His impression at the time was, that it had passed into the cranial cavity; but, judging from the report of the post mortem, he was forced to conclude that the instrument had passed under the dura mater. Two or three syringefuls of the liquid were thrown in, but a good deal of it escaped when the instrument was removed.

Dr. BUCK remarked that, after the removal of the brain, any perforation that had been made through the base of the cranium could scarcely escape notice.

Dr. DRAPER stated that if the injection had really entered the cranial cavity, the appearances said to be caused by it would be the same on both sides; whereas they were described by Dr. Sands as being circumscribed.

Dr. MARKOE did not think, inasmuch as there was no proof that the injection had entered the cranial cavity, that it was very evident that the injection could not be invoked to explain the appearances.

Dr. BUCK.—Was any injection thrown into the great trunks?

Dr. STEELE.—About two quarts and one pint were thrown into the femoral.

Dr. PARKER remarked that there must have been some deception as to the passage of the instrument into the cranium. The turbinated bones were not so easily broken through. He had tried the experiment of introducing an instrument through the cribriform plate, and had failed to succeed.

Dr. STEELE stated that the instrument which he had used was fashioned like a hypodermic syringe, but, of course, of much larger size.

Dr. OTIS asked if it were not possible that the instrument had entered the frontal sinus?

Dr. BRADLEY stated that the point of the instrument, after it had been introduced, could be freely moved.

Dr. PARKER did not see how such a thing was possible under the circumstances—that if it had gone through the cribriform plate of the ethmoid its point would be virtually fixed. He was forced to conclude, in the absence of proof to the contrary, that the injection had, for the most part, passed down the pharynx.

Dr. ELIOT inquired concerning the history of Dr. Conant's case immediately preceding his attack. He had been informed that shortly before his death the doctor had been in attendance upon a bad case of small-pox, which had afterwards become purpuric, and that he had been inoculated, and again that he had a patient with phlegmonous erysipelas, and had been poisoned.

Dr. OTIS remarked that the case of small-pox transpired three weeks before the doctor's death.

Dr. PARKER stated that for some time previous to death, the patient had been much broken down by hard work, especially during the months of July and August, in tending upon cases having typhoid fever, erysipelas, small-pox, &c. It seemed to him that the disease of which the doctor died belonged to that class which we had been in the habit of meeting during the past fifteen years, and spoken of under the head of malignant pustule. It is, of course, not the malignant pustule of English authors; but it is a very peculiar form of disease which is met with more frequently in young persons from twenty to thirty years of age. The face is the seat of the attack, and the lower lip is its most favorite seat. He had never seen the pustule on any of the extremities. It is very often mistaken for erysipelas, but it differs from it, in that instead of commencing in the skin and working its way into the cellular tissue, it first shows itself underneath the epidermis and attacks the periosteum. It first shows itself as a small white spot formed of a creamy substance, which shines through the skin. The color of the surrounding tissues is livid, and they are very hard. It is "cakey" in character, and shows no disposition to diffuse itself. When cut into it is inelastic, and the cut surface presents numerous hardened white spots. It seemed to him to belong to the carbuncular class of diseases, and to require much the same treatment, as far as the knife was concerned, viz., free incision down to the bone. He knew of no remedy except the knife, used early and thoroughly.

Dr. SANDS stated that the few cases of this disease which he had

seen were very distinctly circumscribed, and when there was extension of the disease it was through the medium of the veins. In one case, which proved fatal, this was a distinct feature, and on post mortem examination the coagulated masses in these vessels rolled easily under the finger. In Dr. Conant's case, the post mortem showed the disease to be a sort of erysipelatous mischief, a low grade of inflammatory action without plastic effusion, traveling through the cellular channels of the orbit, and finally involving the brain, as was distinctly shown by the appearance of the pus and false membrane already described.

INFLAMMATION OF THE ŒSOPHAGUS IN AN INFANT—DR LEWIS SMITH.

Dr. LEWIS SMITH presented an œsophagus taken from a child who died the day before at the Infants' Asylum. The patient was a foundling, six or eight weeks old, and when admitted into the institution was placed with a wet nurse, and a few days after was noticed to be failing gradually, and to be losing flesh without assignable cause. The emaciation gradually progressed until it reached that degree which is generally known as marasmus. There was no fever and no acceleration of the pulse, and very little, if any, vomiting. The evacuations of the bowels numbered only two or three daily during its sickness.

A post mortem examination was made, and the only lesion found was an inflammation of the œsophagus. There was no sprue or stomatitis in this case. In conclusion, he stated that during the past summer he had frequently met with cases of children, three or four months old, who had died of this simple œsophagitis. None of these seemed to have any difficulty in swallowing.

TUMOR OF THE RECTUM—DR. WILLARD PARKER.

Dr. PARKER presented a mass removed from the rectum of a gentleman fifty years of age. About six months ago the patient first began to be troubled about the rectum. After each stool a mass would be protruded per anus, and although it would easily return, he was constantly annoyed with the desire to pass something more, and this difficulty increased upon him until he came under Dr. Parker's observation, about two weeks since. On placing him in a position to force it down, a mass of considerable magnitude, vascular in character and red in appearance, was protruded, showing itself attached to the mucous membrane, about an inch and a half above the sphincter, over a space of an inch in diameter in one direction, and three-quarters of an inch in another direction. The wire of the ecraseur having broken during

the operation, a ligature was passed around the base of the mass, which latter was afterwards severed with the knife. Dr. Parker remarked that it was an unusual form of disease of the rectum, and one which he had never seen before. Examined under the microscope, it was found to consist of columnar epithelium upon a fibrous base.

Dr. SANDS stated that he had been able, after a limited amount of research, to find no account of such tumors occurring in the rectum. In Virchow's last work there was a figure of such a growth, which had been found attached to the lining membrane of the gall-bladder of a cow. In structure they resembled the uterine mole.

Dr. PARKER was under the impression that Quain had described a case similar to the one which he had reported.

Dr. DRAPER recollected that Dr. Dalton, some few years ago, presented such a tumor attached to the mucous membrane of the stomach. It seemed to be a sort of villous growth from that membrane.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Renewal of Life: Lectures, Chiefly Clinical. By THOMAS KING CHAMBERS, M.D., Honorary Physician to H. R. H. the Prince of Wales; Physician to St. Mary's and the Lock Hospitals. From the third London Edition. Philadelphia: Lindsay & Blakiston, 1865. 8vo, pp. 638.

The medical profession of this country are under obligations to the American publishers for this reprint of Dr. Chambers' "Lectures," a work whose "time is forever, everywhere its place"—admirable in tone, full of valuable instruction and practical teaching, and written in clear, compact, literate, and, often, epigrammatic English. We can offer but a brief notice of this intrinsically good book, which is certain of finding a wide circle of readers, and we should hope a place in every medical library, and must restrict our comments to a few of the most important topics treated of.

There are many who believe that medicine as a science is falsely so called, and that it can never claim to rank higher than an empiric art. It has been well remarked by M. Cl. Bernard, in his latest contribution to medical literature—"Introduction à l'Étude de la Médecine Expérimentale"—that "beside those who deny medicine to be a science

because there is nothing absolute known in it, we find others who deny it by an entirely opposite process—by admitting that medicine is learnt, one knows not how, and we become possessed of it by means of instinctive science, which they call *medical tact*. Now, it is not to be denied that there may exist in medicine, as in all other practical sciences, that which is called *tact*, or *coup d'œil*. Every one knows that habit may give one a sort of empirical knowledge of things, capable of directing the practitioner, although he may be unable at once to follow the process. But what we regard as blamable is voluntarily to remain in this state of empiricism, and not attempt to get loose. By close observation and study, one can always attain to the comprehension of what one has done, and finally be able to teach others what one knows." The question arises, by what means and on what conditions may medicine become an exact science? By becoming a science of observation and experiment, for it will never be perfect whilst based on observation alone. Its only true basis is physiology, in its most comprehensive sense. It must be made not only the firm foundation, but the key-stone and binding link of our all knowledge regarding the nature of disease processes.

"I am sure" says our author, "that physiology, as it is now taught in our schools, is enough to clear away the remaining mists of these superstitions. They can not stand before the light. To us now health is that nicely adjusted balance of vital functions which is convenient for the uses men put their bodies to, or *ease*; that disordered balance which is inconvenient is bad health, or *disease*. There are no foreign forces to be studied, but simply varied relations arising from deficiency of one or other of the ordinary functions of life, and a consequent want of balance between them. Is it not, then, obvious that the only sure mode of arriving at a knowledge of the deficiencies of vital powers, or diseases, is by a knowledge of those powers of which they are deficiencies? The physiologist is the only true pathologist," (p. 636.)

There are no new modes of nature's acting brought into play by disease; its chemistry is the organic chemistry of health; the same mechanical laws are exhibited; the relations of the material and spiritual world therein are the same. The difference, our author contends, and we are not disposed to join issue with him, "consists in the deficiency—that is, the temporary, or permanent, subtraction—of substance or power, and not in its unwonted increase." Again:

"There are two departments carried on simultaneously—the destructive and constructive; and upon their harmony and completeness depends the perfection of life which we call health. Both are necessary; and the deficiency of either or both, or the preponderance of one

over the other in various parts, or their deficiency in one part while other parts remain active, constitutes a deficiency of life—a disease.

“This deficiency the physician is called upon to remedy; and it is of the utmost importance to his usefulness that he should recognize that it is a deficiency, and act upon the recognition. He must look at his pharmacopœia with this thought constantly present before him, with an eye to the ultimate benefit of the patient, to a goal beyond that of the immediate effects. He should make his chief thought how each of the reagents employed will finally touch life; whether they are calculated to add to or diminish the vital functions, to add to or diminish the vitalized substance of which his patient is made—whether by temporarily diminishing the functions or substance he may not remove an impediment to their balanced actions, so as to lead to a final increase—or whether this artificial diminution of functions or substance may not become permanent, and inflict permanent injury on his patient. This final goal of life renewal must be consciously or unconsciously in the heart of the physician, or in the heart of his guides; otherwise I am sure he contributes more to the ill health than to the good health of mankind,” (pp. 40–1.)

Healthy life consists in a continuous and equally balanced repetition of the two necessary parts of the renewal of the body, constructive and destructive assimilation—in other words, “growing and moulting.” The essential nature of disease, that which makes it disease, is a deficiency of either construction or destruction in one or more points. The cure of disease, natural or artificial, consists in a new birth, or renewal of the deficient life. The morbid phenomena classed under the head of deficiencies of nutrition or form-building are most simply shown by starvation in healthy bodies. Graves, Chossat, Marshall Hall and others have abundantly and satisfactorily illustrated the results of *autophagism*, or the sum of morbid manifestations resulting from prolonged abstinence of food. When a person has nothing to eat, says Trousseau, he lives on himself. Numerous observations have demonstrated that a great number of patients suffering from acute disease have died from intercurrent autophagism, when subjected to a rigorous diet. Dr. Chambers’ remarks, on this point, we can bear witness to from personal observation in the same hospital:

“I have a most lively and painful recollection of seeing, when I was a student in Paris, M. Chomel and others treating pneumonia. I could not at first understand why, in France, so much more marked and more hurtful effects were produced by venesection than in England. At that period we had at home ample opportunities of seeing it practiced; but I never witnessed such prostration follow at St. George’s as I did at the Hôtel Dieu. I was puzzled for a week or two, till at last I noticed that the order for ‘*saignée*’ was accompanied by ‘*diète absolue*.’ I almost doubted my knowledge of French, and was

obliged to ask of the by-standers before I could believe that this meant an utter deprivation of all food ! Here was an immediate explanation of the seeming toughness of my countrymen; for never in our worst days did we carry the Sangrado practice so far as that. Our teachers did not give food enough, but they never bade it to be willfully kept beyond their patient's reach," (pp. 614-15.)

There is much wisdom and practical teaching in the underlying therapeutic views of our author, logical consequences of his doctrine of disease. The object of medical treatment is cure. The end of the physician's labor and care is accomplished by the patient getting well. What is this cure or getting well ? It is a restoration of the disordered body to its natural state of ease or health. It sometimes comes to pass independently of any interference from without, by what is called an effort of nature. Wounds heal, diseases cease in men and animals who have none to help them, the diseased organs resuming their natural functions.

"Now diseased organs must necessarily have less power in them than healthy ones; and it is obvious, therefore, that the curative vitality must plainly be due to, and proceed from, those which remain healthy. It is to their having some sound action to spare that the sick man owes his recovery. The vital force at the same time removes the retained products from the tissues, and replaces them by new material. This is the true teaching of nature's cure," (p. 55.)

Cures by art are effected in the same way. The action of a remedy is a series of vital acts of the body itself. "The true art of healing consists in the establishment of vital acts tending toward the renewal of the body, toward a new birth of deceased tissue, and in the encouragement and aiding of those vital acts after the removal of their original excitant," (p. 56.)

The lecture on "Blood-letting" will abundantly repay careful reading, from its fairness and sensible tone. Our author thinks "it is time to have done with the reactions for and against letting blood, which have been going on throughout the period of the Christian era; the wave which has swelled backward and forward to a dangerous height, ought to settle down into a steady stream. We ought to know clearly *why* we bleed, and then we shall know *when* to bleed."

"The history of opinions on the subject of blood-letting shows us four sects or parties, each one of which has at various times outweighed its rivals in number of adherents. First, the followers and predecessors (for '*vixere fortes ante Agamemmona*') of Hippocrates, who sometimes bled, but always fed, their patients. Second, the disciples of Erasistratus, who denounced bleeding as robbery, and prescribed in its place a complete starvation for several days—*δάτριος ἀσιτία*.

Third, the French practitioners of the two last centuries, immortalized but not checked by the satire of Molière and Le Sage, who both bled and starved at once. Fourth, the followers of Brown, who replaced bleeding by alcohol. From what has been said it will be seen that I should find myself a member of the first named sect, if the unhappy mania of party spirit were again to afflict our profession as it has done of old. And were hero-worship again to become one of our failings, I should probably select as the bible of my medical faith Hippocrates' 'Regimen of Acute Diseases.' For if it might be allowed, out of consideration for the differences between Athens and London, to substitute beef-tea for gruel, there is very little in that capital course of lectures which does not accord with the daily practice of those I think wisest among us in the present day," (pp. 625-5.)

Dr. Chambers classes alcohol as an anæsthetic whose primary action is on the nervous system, and to that action is attributed its effect in staying destructive metamorphosis. A series of experiments made by him would seem to indicate that a moderate dose of alcohol acts by temporarily augmenting the digestive power of the stomach, helping it to appropriate more thoroughly the food; but if advantage is not taken of this first action, its secondary effect is a diminution of the vital functions in general, and digestion among their number. It decreases the excretion of the phosphates, and, as their chief source is nerve-tissue, alcohol must be an arrester of nerve-life, and consequently a controller of nervous action on the rest of the system. The clinical rules which he lays down for the administration of alcohol are, that it is to be given whenever you find the nervous system exhausting itself and the body by an activity in excess of the other bodily functions—when constructive life is low, as in typhoid fever, surgical injuries, low forms of pneumonia, erysipelas, &c. Give it, increase it, leave it off under the guidance of the appetite for food. Divide the daily allowance into two or three doses only, giving enough at once to produce a decided effect. Abstain from its use altogether when the marked feature of the disease consists in the retention of effete matters which ought to be discharged.

In accordance with these general therapeutic views, we find Dr. Chambers' treatment of special morbid states in the main such as will command general acceptance by right thinking practitioners for their soundness and common sense. He puts faith in physiological, tempering, and restorative treatment, and discourages an inconsiderate resort to spoliative and destructive expedients. He stands an alert and obdurate sentry against the fatal fascinations of polypharmacy. "When '*serum lactis*' will suffice, why use *venæ sectio*?" We would particularly indicate the Lectures on Fever, Rheumatic Fever, Pericarditis, Capillary

Bronchitis, and Pneumonia as particularly deserving of careful study by the profession.

We are sorry that the American publishers have thought fit to restore the somewhat fanciful, and to us objectionable, title of "The Renewal of Life," used by the author in the earlier editions, and wisely abandoned in subsequent ones, at the suggestion of his most friendly reviewers. It is to be regretted, too, that the index added to the last—fourth—London edition has not been reprinted in this, by means of which much facility would have been gained for the purpose of easy and frequent consultation.

Lectures on Inflammation; being the First Course delivered before the College of Physicians of Philadelphia, under the bequest of Dr. Mütter. By JOHN H. PACKARD, M.D., author of a "Manual of Minor Surgery," Secretary of the College of Physicians, etc., etc. Philadelphia: J. B. Lippincott & Co. 1865. pp. 276.

The title tells us to what circumstances we owe this little book. By an agreement entered into in 1858, the late Dr. Mütter bequeathed to the College of Physicians of Philadelphia—the oldest association of American physicians—his pathological museum, together with a fund for its preservation, and the endowment of a Lectureship. The College appointed Dr. Packard to deliver the first three courses of lectures under this bequest, and we have here the first series. Dr. Packard says: "In them I have endeavored to set forth the subject of Inflammation in the light of modern pathology. As a matter of course, within such limits it would be vain to attempt to exhaust a topic of such magnitude and importance; but my aim has been to take up the principal points in regard to it, and to give a plain and succinct history of their present aspect." (Preface.)

How far the author has been successful will be appreciated by those who may read the work. We do not think that he has been fortunate in the choice of his subject, which, to be intelligently and attractively treated by the "light of modern pathology," requires more ample handling. While the "Lectures" fairly present the views of Mr. Paget and Virchow on the subject of inflammation, as set forth in their writings, we can hardly say that Dr. Packard does full justice to all their co-laborers in this branch of pathology during the past twenty years, and who have really given such important *momenta* towards the development of the current theory of Inflammation. The views of Mr. Paget, which Dr. Packard quotes from the first edition of his "Lectures on Surgical Pathology"—originally delivered before the Royal College of

Surgeons during the six years between 1847-'52—merely represented those of the German school of a quarter of a century since, and to it, and not to the English surgeon, should the credit be given. We look in vain for the names of Lister, Simon, C. J. B. Williams, Bennett, Goodsir, Toynbee, Redfern, Parkes, Beale, and many others in Great Britain, and a number in Continental Europe, to whose studies on this subject we are indebted for substantial progress. The valuable researches of Mr. Goodsir, published in 1845, in which he maintained that disintegration of inflamed tissue results from dynamical disturbance of the tissue itself, that veins and lymphatics, in reference to ulceration and absorption, are mere ducts for the conveying away the products of action, and that ulceration involves profuse endogeny and shedding of germs, were a most important step towards the comprehension of the pathogeny of inflammation, and should not be overlooked by any one who undertakes to present a digest of our knowledge of the subject. The same may be said of the want of mention of the observations and experiments of Mr. Joseph Lister, Professor of Surgery in the University of Glasgow, presented to the Royal Society in 1857, and the most directly valuable contribution to the study of inflammation, to our mind, made for a long time, and which perfectly illustrate the series of complex changes through which the inflammatory process is seen to move, and especially the cause of the "stasis" and exudation of the liquor sanguinis, phenomena hitherto of such difficult and variable explanation.

Dr. Packard tells us "that a part which is inflamed has its temperature raised above the normal standard for that part can not for a moment be doubted; but the degree of this elevation is generally much less than could be supposed either by the patient or by the observer." "Certain it is that the thermometric changes produced by this condition are much slighter than would be supposed. Hunter held the view that the temperature of an inflamed part never exceeded that of the central portions of the body, and the statement is confirmed by Andral and Gavarret, and by most other writers," (pp. 59, 60.) Now, does this statement fairly represent the state of our knowledge on the subject, and what do we find "*in most other writers?*" We have no mention made of the experiments performed in 1835 by Messrs. Becquerel and Breschet, which went to prove that an inflamed part was actively calorific—that it is, or tends to be, warmer than the blood which supplies it—in opposition to Hunter, who believed that an inflamed part is passive in the alleged change of temperature, which can not exceed that of the blood which supplies it. We believe that no value can be

attached to either set of experiments; and the question remained open for decision whether "heat is a symptom of inflammation only in external parts, and only as a passive result of their over-fullness with blood? or is the inflamed part actively productive of heat?" Its solution, we are of opinion, has been practically made by the thermo-electric observations made in 1860 by Mr. John Simon and Dr. Edmund Montgomery, of St. Thomas' Hospital, and which go far to prove satisfactorily that an inflamed part is no mere passive recipient of heat, but is itself actively calorific. We have among the observed results, *first*, that the arterial blood supplied to an inflamed limb is found less warm than the focus of inflammation itself; *second*, that the venous blood returning from an inflamed limb, though found less warm than the focus of inflammation, is found warmer than the arterial blood supplied to the limb; *third*, that the venous blood returning from an inflamed limb is found warmer than the corresponding current on the opposite side of the body.*

We are told that the system at large passes into a febrile condition under the influence of local disturbances, and, "as one element of this disorder, the temperature of the whole body rises in some degree." Surely our author, in speaking of the increased temperature of the blood in the constitutional affection, which runs its course in connection with local inflammation, and is known by the name of "inflammatory fever," might have alluded to the positive contributions of Montgomery, Parkes, Ringer, Wunderlich, Traube, Bilroth, van Bäremsprung, &c., on the thermometry of febrility, and who, with the aid of exact thermometric observation, have scientifically established so many facts of practical use, both in idiopathic fevers, visceral inflammations, and surgical disorders, tending to demonstrate that increased bodily calorificity is a uniform attendant on febrility, whether sympathetic, continued or intermittent; that it is as constant in ague and hectic as in typhus or pneumonia; that febrile rigor denotes a more rapid heating of the blood, and as it occurs in ague and pyæmia, is distinctively the sign of a sudden rise of temperature.

Materia Medica, for the Use of Students. By JOHN B. BIDDLE, M.D., Professor of Materia Medica and General Therapeutics in Jefferson Medical College, etc., etc. With Illustrations. Philadelphia: Lindsay & Blakiston. 1865. 8vo, pp. 359.

This is a second edition of the author's "Review of Materia Med-

* Holmes' System of Surgery. Article, "Inflammation." Vol. i. London, 1861.

ica," revised, enlarged and adapted to the United States Pharmacopœia of 1860. It belongs to a class of books which severely try the critic's charity, their contents being, for the most part,

"Drawn from the mouldy rolls of Noah's ark,"

embalming the relics of a by-gone age, and ignoring all scientific progress. The catalogue of veteran stock-remedies which we have here spread before us demands no small stock of courage to face, and we soon become satisfied they should long ago have been mustered out of service on account of general worthlessness, and that their retention is a fraud on the junior members of the profession and the public.

Professor Biddle states that "numerous additions have been made to the list of articles treated of, and the work has been remodeled and, in many parts, rewritten. Although not designed to take the place of the more voluminous and systematic treatises upon the subject, it is believed that it will be found to contain a succinct account of all the articles of the *Materia Medica* in use in this country." Yet we have looked in vain for mention of the bromide of ammonium, cerium and its salts, pepsine, Calabar bean, (physostigma venenosum,) apiol, carbolic acid, the alkaline and earthy sulphites, and other recent remedies now in general use.

We will extract a few plums from our author's pudding:

"Bromide of potassium has been used as a substitute for the iodide in bronchocele, scrofula, chronic cutaneous affections, secondary syphilis, &c., but it is inferior, in these diseases, to the iodic salt. It has, however, proved a very efficacious remedy in diseases of the nervous centres, as whooping cough, infantile convulsions, and especially epilepsy, over which it is now believed to exert more control than any other article of the *Materia Medica*. It is found also to be the most efficient remedy which we possess in allaying venereal excitement, and hence its employment in nymphomania, chordee, &c., and as a preventative of masturbation in prisons, barracks, &c. Dose, from three to five grains, several times a day," (p. 274.)

We have here no mention of the wonderful anæsthetic powers of bromide of potassium over mucous membranes generally, and those of the pharynx and larynx especially—thus becoming a powerful aid in laryngoscopic examinations and operations; of its repeatedly attested value in enlargement of the spleen; its prompt efficacy in removing congestive headaches; and its positive hypnotic qualities. Its manner of administration is given very vaguely.

Of the uses of muriate of ammonia Prof. Biddle says: "It is not much employed in Great Britain or the United States, but it is exten-

sively used in Germany as a refrigerant sedative in mild fevers, attended with stoppage of the secretions—as a resolvent in organic enlargements—in amenorrhœa, and in catarrhs, urethritis, &c. Dose, gr. v.—xxx. every two or three hours, in powder or mucilaginous solution,” (p. 283.) This salt is very largely employed in Great Britain, and has been for the past quarter of a century, and we believe the same to be true in this country. Its value, in certain forms of hemicrania—the only instance in which we know of its administration in thirty-grain doses being recommended—is entirely overlooked, as well as its recognized efficiency in parenchymatous and mucous inflammations, in small and repeated doses. Many physicians use it in their practice to the utter exclusion of mercury, and, from a long and extensive experience with it, we look upon it as invaluable in gastric, bronchitic, pneumonic, and hepatic derangements, and have seen it repeatedly act with miraculous results in acute iritis.

Admirers of our indigenous podophyllum, so well known and so generally used, will think that it deserves something more than this “curt epitome.” “This is an active hydragogue cathartic, analogous in its operation to jalap, for which it might very well be substituted. It is an ingredient in several cathartic nostrums,” (p. 212.) No word for it as an accredited cholagogue. In summarizing the employment of ipecacuanha, no mention is made of its success in large doses in dysentery, so well proved within a few years in British India and in our own army.

We regret to have felt it our duty to indicate the general defects of this work, and a few of its shortcomings. The difficulty lies in the nature of the book itself, more than in defective execution. We can not, with impunity, turn our back on the rapid strides that physiology, pathology and chemistry have made and are making. The substantial progress of medicine must be owned. Such is not the scope of a work like this, which, instead of an aid, is a hindrance to the spread of real knowledge, encouraging the pouring in hap-hazard of the most powerful and dangerous drugs.

The Practice of Medicine. By THOMAS HAWKES TANNER, M.D., F.L.S., etc., etc. From the Fifth London Edition, Enlarged and Improved. Philadelphia: Lindsay & Blakiston, 1866. 8vo, pp. 835.

The well known “Manual” of Dr. Tanner is now expanded, in the fifth edition, to a volume of over eight hundred pages, with the more ambitious title of “The Practice of Medicine,” and yet it only remains a Manual; but, with some insignificant imperfections, a very good

Manual. The author, in attempting so much, incurred the serious risk of compromising the usefulness of a work of intrinsic value. Dr. Tanner, in the present compilation, displays great industry, intelligence and practical knowledge. His descriptions are terse and clear, and fairly representative. His therapeutic fertility will delight the student, and satisfy the young practitioner. There is an "Appendix of Formulæ," marshaling several hundred prescriptions, which will probably prove the most attractive feature of the work. The chapter on "Climates for Invalids" is well condensed, and contains in few pages much valuable information on a subject generally but little understood; and the same may be said of the concluding one on "Mineral Waters."

Without invidious prejudice to the excellent handling of many important diseases in the "Practice of Medicine"—for it is quite impossible for us to follow Dr. Tanner through his bulky volume—we would indicate "Pneumonia" as written in the author's happiest manner. His remarks on the treatment are sensible and just, and go as far as our present knowledge permits.

"Bleeding, tartar emetic, and mercury, are the agents on which we have been mainly taught to rely; but these remedies will, I feel convinced, do much more harm than good if applied to the treatment of pneumonia in the present day. It is the more necessary to insist upon this point, because some of our text books still advocate depletion. In recommending the adoption of a very simple line of practice, I am only doing that my experience has taught me is much the best, not only for the ultimate safety of the patient, but even for diminishing the duration of the disease. I am quite alive to the argument that whereas our ancestors bled too much, we may fall into the opposite error and bleed too little; but whatever may be said upon this head it can only be replied, that the practitioner is advised not to have recourse to anti-phlogistic remedies in the treatment of pneumonia, because it is firmly believed that their use always retards and often prevents restoration to health," (pp. 363-4.)

The work is well printed in large, clear type, on thick, white paper. Indeed, all the medical publications of Messrs. Lindsay & Blakiston are issued in a most creditable manner.

Chloroform; its Action and Administration. By ARTHUR ERNEST SANSON, M.B., London. Philadelphia: Lindsay & Blakiston. 1866. 12mo, pp. 279.

The subject of this volume is one which is always attractive to practical medical men, requiring almost daily consideration, and involving great responsibility. The author was educated in a good school, the

King's College Hospital of London, under Fergusson, Todd and Johnson, and was associated frequently with the late Dr. Snow, the author of the most philosophical and exhaustive work on the same subject. His object in the present work is to present a "brief resumé of our present knowledge of chloroform and its effects;" and, on perusal of his book, we are free to say that he has accomplished it in a highly creditable manner. He has stated concisely and ably all that the student and practitioner requires to know of the history and applications of the great remedy, introducing nothing novel or original, but, as far as we observe, omitting nothing that is useful. He prefers chloroform to ether as an anæsthetic, and seems to have had no personal experience in the use of the latter, suggesting that a statue should be erected to Prof. Simpson, and seemingly not disinclined to lose sight of the fact that anæsthesia was given to humanity by America.

Our author considers that the principal dangers attending the administration of chloroform are to be averted by employing its vapor for inhalation in a state of dissolution, and by means of an inhaler, and describes one of his own invention for this purpose. British opinion seems to be unsettled upon this point, as we notice Mr. Lister, in Holmes' recently published "System of Surgery," expresses his conviction that "the cloth" is as safe as any apparatus. Dr. Sansom also thinks favorably of diluting chloroform with alcohol under certain circumstances, and "is quite sure that the administration, by the stomach, of a little alcoholic stimulant acts beneficially."

His discussion of the modes in which chloroform causes death, and of the means of avoiding accidents and of effecting resuscitation, is concise and exhaustive.

In conclusion, we think that this work is well calculated to be useful and popular with the medical profession.

Schönberg's Improved Map of the United States and Canada, carefully compiled from the latest and best Authorities. New York: Schönberg & Co. 1866.

There is no better sign of educational progress than the growing popular demand for maps. Geography is one of the most important handmaids of history and general knowledge. The map now before us is the latest publication of its kind, and embraces the most recent geographical and hydrographical points of interest, including the principal battle-grounds, together with railways and canals, and affording a full and convenient means of reference. The map is 5 ft. wide and

5½ ft. long, and extends from Halifax on the East, to the Pawnee Reservation and Galveston, Texas, on the West; and from the mouth of the St. Lawrence on the North, to the Florida Keys and Nassau, N. P., on the South. It is on the large scale of thirty miles to the inch. These geographers are not like those described by Plutarch, in his life of Theseus, who "crowd into the edges of their maps, parts of the world which they do not know about, adding notes that all beyond lies nothing but sandy deserts, full of wild beasts and unapproachable bogs;" for the marginal space we find filled with the western portion of the United States and British Provinces, from the 94th degree, W. L., to the Pacific Ocean, California, Nevada, Oregon and Texas, in counties, the world on Mercator's projection, comparative height of the principal mountains in the world, with a variety of interesting statistics, etc. The projection is rectangular polyconic, the scale is of such size as to admit of fullness and distinctness, the authorities consulted are the best, the drawing has been carefully and critically made, the engraving is clear, the names legible, and the whole work is in the most creditable style. We can recommend it to our readers for trustworthiness and easy reference.

PROGRESS OF THE MEDICAL SCIENCES.

I.—DISEASES OF WOMEN AND CHILDREN.

1. *A New Mode of Effecting Delivery.*

Although the various measures in which consists what M. Senn, of Geneva, calls his method, are to be found described in all works on obstetrics, yet their simultaneous application is, in reality, a suggestion which originates with M. Senn. The question of prior invention is here, however, of no import; the system is a sound one, and we conceive it our duty to make it known. The following remarks on the subject, by the learned practitioner we have named, are borrowed from the pages of the "*Révue de Thérapeutique*."

This method, adopted of late years by M. Senn, and which appears to him to afford all desirable security, is founded on practical experience and logical induction, and is a combination of three elements:

Injection into the placenta of the blood contained in the cord, and of the blood of the infant; gradual and permanent pressure of the abdomen by means of a very simple bandage; and entire immobility of the mother in the horizontal attitude for two hours after delivery.

As soon as the child is born, the accoucheur grasps the cord at about an inch from the navel, in order to protect the latter from any injurious dragging, and with the finger and thumb of the other hand

forces back into the placenta the blood contained in the funicular blood-vessels.

By pressure with the finger and thumb the blood is thus forced back and retained in the placenta, while the fingers of the other hand are loosened, and allow the arteries of the funis to fill. The operator then again presses on the cord, near its insertion at the navel, and the injection is repeated with the other hand. This procedure should be renewed three or four times, according to the condition of the child, its more or less plethoric appearance, its screams, &c. Of course, in the case of an anemic infant this plan should never be attempted.

In every instance, M. Senn performs this little operation so promptly as to prevent the mother being aware of it. When the injection has been completed, and the placenta is filled with blood, a ligature is placed on the cord, between the vulva and the fingers of the surgeon. It is then divided half way between the umbilicus and the ligature, the child is removed, and the cord secured as usual.

Thus, artificially tumefied, the placenta becomes incapable of following the retraction of the womb, its attachments are necessarily loosened, and the process of delivery is restored to its most desirable conditions, viz., the simultaneous expulsion of the infant and of the afterbirth. If the organ is retained more than five or six minutes above the vulva, it can easily be removed by gentle traction.

The second measure advocated by M. Senn is the permanent and gradual compression of the abdomen. The pressure is not effected with the hand, but with a soft cloth in several folds, previously laid under the woman's loins in the early stage of labor.

"During the progress of labor," says M. Senn, "the extremities of the cloth are folded under the mattress. But immediately after the birth of the child, I rapidly ascertain the situation and size of the uterus, and wrap one side of the cloth round the abdomen, while the nurse draws the other extremity horizontally towards her, in order to support the abdomen, to give a fulcrum to the muscles, and maintain the womb in its position.

"The nurse then hands over to me the extremity of the cloth which she previously held, and with this I again envelop the abdomen. The tightness of the bandage should be gradually increased in proportion as the size of the body diminishes, first on the expulsion of the afterbirth, and again after an interval of half an hour, when it should be permanently secured with pins. This dressing prevents all displacement or dilatation of the uterus; the viscus can neither rise in the abdominal cavity, nor be distended by coagula, and internal hæmorrhage becomes impossible on account of the permanent pressure, which forms an unconquerable obstacle to its occurrence.

"The venous circulation is also thus promoted by the support given to the body; the circulation in the venous system is rendered more active, and the chances of syncope are thereby greatly diminished."

With regard to the third measure recommended by Dr. Senn, viz., perfect immobility in the horizontal attitude, it is imperatively required by the necessity of avoiding any cause likely to disturb the first fibrinous coagula which fill the vagina and prevent unnecessary loss of blood. The head of the patient should lie as low as possible, and for two hours after the conclusion of labor she should drink from the sucking-bottle, so as to render all muscular effort unnecessary.

If, after an interval of eight or ten minutes, the placenta is not found lying in the vagina, M. Senn conceives that it must be attached to the womb by morbid adhesions, and its removal should at once attempted. But even under these unfavorable circumstances, delivery

will always be effected in less than a quarter of an hour after the expulsion of the infant, and all danger of flooding will have been averted.—*Medical Circular*.

2. *Remarks on the Diet suitable after Child-birth.* By HENRY LOWNDES.

When I began practice, and for some years, I used to go by those precepts that almost all agree in, giving gruel and other thin slops for the first few days; and I can remember what repugnance many patients had to this substance, which resembles no ordinary article of human food in this country. I had to preach sermons to them on the extraordinary virtues of this—I do not know what to call it. We are told by Edgar Poe of beings that are “neither man nor woman; they are neither brute nor human, they are ghouls;” and this material is neither food nor drink, it can only be described as gruel. It can neither satisfy the cravings of hunger, nor minister to the sense of taste or that of smell. It is unknown who first invented such an unlikely article of diet. However, I found, in many cases, that patients would not be persuaded by my eulogies of this preparation, and would sometimes take broth, sometimes beer, and sometimes meat, and would confess to it, and certainly used to look all the better. Gradually, whether it was from this experience, or from something I had read, or from seeing some one else’s practice, I can not tell, I began to try a good diet, without over-stimulating. For at least three years, I have acted in nearly all cases, except after first labors or labors unusually severe, on the following system.

I give some stimulus, either wine or ale, directly after the labor is over. On the day of confinement, I restrict the patient to tea or (if she likes, indeed) gruel, with toast or bread and butter.* On the following day the same, with a large breakfast-cup of good strong broth for dinner. The third day, a small lean chop or a little chicken. The next day the same, with a little wine and water or ale, if the patient be accustomed to these beverages. Of course, all cases can not be treated alike, and a lower diet may sometimes be necessary; but I much more frequently find it useful to begin with stimulants rather sooner than I have indicated.

In first cases, and in cases of severe protracted labor, we know that the soft parts must have undergone great pressure and bruising; and I generally keep these patients a longer time without alcoholic stimulus.

I shall not trouble you with a series of cases and their results; but I can say, with confidence, that I have met in no case with any ill consequences following the mode of diet I have mentioned. It is true that I have seen mischief occur in more than one case where the patient has thought, because she might have a chop for dinner, she might therefore have one for supper also, and so forth; but the evil results of excessive indulgence are not to put a stop to all moderate use of good things. By this mode of diet, I believe the patients escape, in a great measure, those muscular pains that are so frequent when a patient begins to move about, and also those affections of the breast and nipple, in which the pain is out of all proportion to the actual lesion.

As it is always agreeable to make one’s theories and practice agree, I will submit a few considerations in favor of a more “feeding” system.

Supposing, for a moment, that we even take labor to be what it has been called, a severe traumatic lesion, do we condemn a patient that has suffered from a compound fracture to an immediate course of gruel

*In addition to these I find a little bread and milk very desirable.

and tea? and if we did adopt such a course, should we render him more likely to escape, or better able to bear, the inflammatory action that will sooner or later occur? Experience proves the reverse. I will speak again of a severe compound fracture. For some time after the accident, no change of any consequence takes place in the damaged parts; they, in common with the whole system, have received a shock. In a varying time, inflammatory action shows itself in the part, and if the external wound do not close at once, that action will be violent and the whole system suffer great febrile disturbance. Now, a cooling and restricted diet may be necessary; but the patient will be much better able to bear the shock of the inflammation, if that first interval of repose has been employed in overcoming, by suitable diet and stimulants, the shock of the accident. Indeed, nothing is so certain to bring on one evil, traumatic delirium, as too low a diet after a severe surgical shock.

There are, however, many points that sufficiently make a distinction between the process of labor and a severe traumatic lesion; and I need, I think, only refer to one, viz., the absence after labor, as a rule, of any inflammatory reaction.

Then, instead of considering labor as a severe surgical accident, let us consider it in the light of a great physiological process; one of those processes in which the voluntary and reflex nervous systems are both concerned. No one would think of calling the act of micturition or of defecation a traumatic lesion, and yet these acts seem hardly to differ from the act of labor, except in degree and in frequency of occurrence. The frequency of these troublesome acts deprives them of the terrors that might perhaps invest them if, like labor, they came at distant intervals. The act of sneezing may be quoted as a most alarming convulsive phenomena to which we have got quite used.

Labor differs from these other actions in being generally of a much more severe character; it is often accompanied with much severe muscular effort, and it is necessarily followed by much fatigue. This fatigue, and the waste of tissue that has taken place, must be recruited by stimulants and nutriment; and these, I believe, in moderate amount, may be given during labor when that is long continued, and immediately after labor, with the greatest advantage.

Puerperal fever is perhaps the only complication of an inflammatory character that we need have much fear of during the first few days; and this disease is of so low a type, and bears so much resemblance in its nature to typhus and typhoid fevers, that we may very well suppose that, like these, it will be more apt to attack those who are in a low feeble state than those who are in better condition. That good feeding does not conduce to this disease, my own limited experience would certainly show.

Beyond the evidences of its contagious nature, the causes of this insidious and fatal disease seem wrapped in obscurity; but we suppose that a good diet and a cheerful mind must be powerful prophylactics to those exposed to this contagion.

At one time, all the apertures for the entrance and exit of air from the lying-in room were carefully stopped up, on the idea of the patient being now especially liable to suffer from cold; bed-clothes were heaped on, and at one time spiced cordials were diligently given. Now we take pains to ventilate the room well; but, while we do not keep off the access of cool air, we are very apt, if we keep to another part of the old system, to have our patient in a very unfit state to bear the air. For a constant course of gruel in vast quantities and tea relax all the tissues, and the skin among the rest. The female acquires, after a

time, a perfectly sodden appearance, which is quite characteristic. The flesh pale and soft and moist; the abdomen full of flatus.

But we are told that such a diet is good for the flow of milk. In the early period of lactation, however, there is likely to be too much rather than too little milk; and with regard to the quality of that milk, if we compare it with other secretions, we must suppose that it will be most perfect in its character, and so most suitable to build up the child, when the general health of the mother is in its most perfect state. And here I would say a word of milk-fever, which perhaps I ought to have mentioned before. We read much of it; but I believe that, except in primiparæ, the secretion of milk commences without any febrile phenomena whatever, and in the case of primiparæ, I have already explained that other causes lead us to adopt a rather more severe regimen than in other cases.

The whole subject of diet is so extensive, for it must be adapted to the season, the constitution, and the habits of life, that I shall be excused if I have only on this occasion dipped a little into the matter; and I shall be glad to know from those who have a more extended experience in midwifery what line of practice they adopt; and whether there are any who still adhere to the rigid rules that we seem to have learnt rather from Celsus, or some of his predecessors, than from Nature.—*British Medical Journal*.

3. *On the Causes Endangering the Life of the Fœtus During Labor.* By C. K. H. PATERSON.

In general domiciliary midwifery, it will not, I trust, be disputed for a moment that occasionally is witnessed the occurrence of, or if not, the tendency to "hazardous states," in certain kinds of labor, calculated to endanger foetal life. Doubtless not a few labor cases during a year, when seen at the first visit by the obstetric practitioner, are, to the best of his judgment, in their early stage favorable; while frequently, when an examination has been made later, they have become either difficult or complicated labors. Accordingly, it will be allowed, from the occasional uncertainty of the result in the latter, that it is an important and valuable attainment in midwifery practice to be able to make a right discrimination between the nature of the presentation, in each individual case, from that of others known to happen, as well as to determine in time when to proceed in aiding the parturient woman. Whether it be in administering needful nourishment or other means internally, or by the mouth, or by having recourse to artificial interference with the same end in view, namely, to procure her delivery safely for herself and child. Having thus premised, I shall now proceed to state my views in detail, though briefly, as follows:

How far the tendency, oftentimes in difficult and complicated labors, to endanger foetal life may be obviated, must undoubtedly depend much on the kind of presentation, and how such may be managed. The putting off too long, or not sending for proper medical assistance in time, often endangers foetal life.

The management of cases of lingering or difficult labor, although there is no occasion for over-anxiety, provided they are seen in time, and when properly attended to, often ends well to the fœtus. Cases of difficult labor have hitherto often come under my notice, with cephalic presentation chiefly, not a few of which, as might be expected, were accompanied in primiparæ with rigidity of parts, and as the labor difficulty gradually went on, owing to the os uteri and other parts taking in some of these a much longer time than ordinary to

dilate and relax before the foetal head was sufficiently advanced for delivery, notwithstanding previous means were used internally.

Where, on the other hand, there were present (rendering the labor for the time they lasted more than usually difficult) signs in maternal passages of coiling of the foetal cord round the neck, and when ergot of rye was early made use of, and before the head was pretty well advanced, I have seldom seen the expected results accruing from the latter favorable to the foetus. In my own practice, coiling of the cord round the neck or body of foetus has been found only in the child-births of multiparous woman, and I have observed such an occurrence frequently to endanger foetal life in no small number of cases.

Misplacement or unnatural shortness above of the cord, when the foetal head is in the pelvis, and there is found plenty of room for the latter to pass through, may be suspected to exist, if, during the continuance of pretty strong and regular pains for some time, the head slowly advances in a given time, and it is ascertained that it retreats after each pain, while the os uteri has become well dilated. Nevertheless, and although relying a good deal on the above sign of the existence of twisting of the cord, I have used ergot less than formerly, preferring non-interference instead of pushing cases of this kind when there was no urgent symptom calling for active and immediate aid, and I have never had to regret doing so. But should the cord be supposed to be round the neck of the foetus, and notwithstanding it may be the chief cause of retarding the progress of the labor, when not continuing beyond a reasonable time, it is even safer to omit than to have recourse to ergot, and there is less danger to foetal life, except the labor is near its termination, and then it may be found very serviceable at times. Still there are cases, doubtless, in which it is often necessary to give ergot, and with excellent results, as when the labor is hindered by insufficient uterine action—membranes previously broken—the os uteri being well dilated at the same time, and also ample pelvic room, no discordance between the bony parts and foetal head, the latter having descended to the lower pelvic outlet or hollow of the sacrum. While, on the contrary, when ergot is given too early, or sometime before delivery can be safely effected, or after the liquor amnii has flowed away spontaneously and prematurely, or has been too soon let off artificially, and the os uteri not much dilated, nor soon dilatable, the head not advancing in proportion to the length of time the parturient woman has been in labor, and more especially if, after administering ergot, the labor is not within a reasonable time afterwards terminated, or should it be allowed to linger on instead of adopting artificial interference as soon as the case is favorable for or demands it, and the measure is not had recourse to, at a period of delay of this kind, it is rare, in my humble experience, to have a live child born in a labor managed as just described. Hence, when the aforementioned signs are found in the maternal passages of the hindrance of the foetal head, during labor, from coiling or twisting of the umbilical cord round the neck or other parts of the body of foetus, as the cord must consequently be much shorter from this circumstance, but if not so, at any rate it is generally longer in its duration and more difficult; and, to my mind, it has seemed to be both safer and better practice to omit ergot, in such kind of cases, until towards the close of the labor, on account of the occasional subsiding of the maternal pains, when, at this stage, its administration in the usual form and doses will tend less to endanger foetal life, and it will also shorten the labor.

In reference to the above remarks, my usual practice for years past has been, and is still, to *let Nature, for a reasonable time, do her own work*, and to render extra aid only if requisite for the safety of both mother and child.

I shall merely, at present, add a few of the causes in the maternal passages that have conduced more or less to endanger foetal life in cases attended by me, viz.:

The foetal head, when it had rested too long on an unyielding perineum, or when there is rigidity of parts, especially early and late in life, (first pregnancies at full time.)

In presentation of the head, it being only expelled, and while immediately after its expulsion is attended with too much delay in delivery, on account of want of sufficient pains to expel the shoulders when large.

In flooding and breech cases from not getting the head, when large, speedily delivered. In unusual projection of the promontory of the sacrum, in preventing the head from descending in consequence of too little space.

Foetal head too long delayed in pelvis during labor, from inefficient uterine action, or unusual rigidity of one or more parts.

Prolapse of cord by side of head, or coiled round the neck or other parts of the body of foetus.

Foetal umbilical hernia when large.

Foetal head (male) firmly ossified, and also rather larger than ordinary, and I may mention, in addition, maternal mental impressions.

With all these complications, we have certainly a wide field for highly important and valuable investigation, as well as interesting questions, obstetrical and physiological, and especially in a medico-legal point of view.—*Medical Circular*.

4. *Mortality of Childbed as Affected by the Number of Labors.*

Dr. Matthews Duncan, in a carefully prepared statistical paper, gives the following as the result of his investigations. 1. The mortality of first labors is about twice the mortality of all subsequent labors taken together. 2. The mortality from puerperal fever following first labors is about twice the mortality from puerperal fever following all subsequent labors taken together. 3. As the number of a woman's labor increases above nine, the risk of death following labor increases with the number. 4. As the number of a woman's labor increases above nine, the risk of death from puerperal fever following labor increases with the number. 5. If a woman have a large family she escapes extraordinary risk in surviving her first labor, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.—*Edinburgh Medical Journal*.

5. *Precocious Puberty.*

M. Ramon de la Sagra related, at a meeting of the Academy of Sciences, the case of a negro child who, at birth, was observed to have the breasts much developed, and in whom, a few months later, a sanguineous discharge from the genital organs took place, and, in the second year, recurred at regular monthly intervals. When M. Ramon de la Sagra saw her, at the age of thirty-two months, she had passed through dentition favorably; and the throat, and the genital organs,

which, as well as the axillæ, were covered with a fine down, gave her the appearance of a girl of thirteen of the negro race. M. Ramon de la Sagra continued to see the child up to the age of seven, at which time she was in good health, the development having steadily continued.—*Gaz. Méd. de Paris.*

II.—MATERIA MEDICA AND THERAPEUTICS.

1. *Notes on the Use of Astringents, Stimulant-Astringents, and Caustics, in Affections of the Eye.* By D. ARGYLL ROBERTSON.

There are no remedies which are so much, and but few too often indiscriminately, employed in diseases of the eye, as astringents, stimulant-astringents, and caustics. Under these heads are included sulphate of copper, nitrate of silver, alum, corrosive sublimate, and the active ingredients of almost all the ordinary collyria. They may conveniently be divided into the three classes I have already specified, for although many of them possess properties enabling them to be ranked under all the three heads according to the strength of the preparation employed, yet some possess, in a higher degree, the one, some the other action. This division is of essential practical importance, for cases constantly occur in which the use of a stimulant-astringent application aggravates the disease, while the employment of an astringent preparation rapidly effects a cure. On the other hand, many chronic inflammatory affections yield much more rapidly to a stimulant-astringent than to a purely astringent lotion. It is almost needless to observe that the use of a caustic solution to such a tender organ as the eye is applicable only in a few special cases.

Under the term Astringents, I would reckon solutions of tannin (gr. v. ad. ʒi.) or acetate of lead (gr. i—ij. ad. ʒj.) drops of nitrate of silver (gr. ii. ad. ʒi.) and infusion of tea (ʒʒ. ad. Oi.) A remedy I have found an excellent astringent, and for which I am indebted to my friend Dr. Andrew Inglis, is the resin of the *Argemone Mexicana* (yellow thistle) dissolved in glycerine. It is, I believe, much employed by the native oculists of India. A weak ointment of the red oxide of mercury, consisting of one part of the Pharmacopœial ointment and seven of lard, well mixed, also forms a useful astringent. The class of stimulant-astringents includes solutions of sulphate of zinc (gr. i—iii. ad. ʒj.) alum (g. iv. ad. ʒi.) sulphate of copper (gr. i. ad. ʒi.) corrosive sublimate (gr. i. ad. ʒvi.) nitrate of silver drops (gr. x—xx. ad. ʒi.) and vinum opii, either pure or diluted, with equal parts of water.

As caustics, the solid nitrate of silver, either pure or fused, along with nitrate of potass, (either equal parts or two parts of the latter to one of the former,) as recommended by Professor Von Graefe, and a crystal of sulphate of copper, are those most usually employed.

I have now only a few suggestions to make regarding their use. I would first remark that the employment of all the three classes of remedies should be limited almost exclusively to the most superficial affections of the eye. Thus we find them of most service in inflammatory affections of the conjunctiva. In the earlier stages of acute inflammation of that membrane, great benefit will be derived from the use of purely astringent washes, while in the chronic stages, and in its chronic inflammatory affections, the stimulant-astringents are more particularly indicated. In the common affection, granular lids, it is advisable to vary the application according to the nature of the granu-

lations; in those cases in which the granulations are large, soft, and flabby, consisting of the papillæ of the conjunctiva much distended and highly vascular, and in which the whole of the conjunctiva is congested and thickened, the use of strong astringent washes, such as those of tannin or nitrate of silver, are of most use; whereas, in other cases in which the granulations are small, hard, and light colored, more stimulant applications, such as a crystal of sulphate of copper, rubbed gently over the granular surface, answers best.

In affections of the cornea, astringents and stimulant-astringents must alike be used with great caution. They should never be used in acute inflammatory affections of that structure, and they must be used very cautiously, even where the inflammation is chronic and superficial, where their application is frequently of great service. If used too early or too freely, inflammatory reaction is certain to occur, and the affection aggravated. The same rule applies to ulcers of the cornea. The application of cold water is a good method of testing whether the eye is in a condition to bear the use of these remedies or not. Where its application is grateful to the patient, the use of mild astringent washes will generally be found to answer well. I may here repeat a caution which is to be found in all ophthalmic works, to avoid the use of lead washes in ulcers of the cornea, as the chloride of lead, which is formed by the union of the wash with the lachrymal secretion becoming deposited at the foot of the ulcers, forms a permanent opacity. To hasten the absorption of corneal opacities, a weak stimulant-astringent wash may be freely used.

Stimulant-astringents and astringents should never be employed in iritis, or any of the deeper-seated inflammations of the eye; as, so far from doing any good, they invariably aggravate the disease. In fact, the worst cases of iritis a surgeon can be called upon to treat, are those in which such lotions have been employed in the earlier stages.

Caustic applications are not required in many affections of the eye. I have already referred to the use of sulphate of copper in one variety of granular lids. They may also be employed in chronic enlargement of the caruncle—in eversion of the lids from inflammatory thickening of the conjunctiva, and to remove the fungoid growth which often forms at the conjunctival wound after the operation for strabismus; but in most of these cases the knife or scissors are to be preferred. In a case of chronic fistula of the cornea, I found the application of a fine point of the solid nitrate of silver induce closure of the aperture. Caustics are occasionally employed in prolapsus iridis to remove the prolapsed portion of the iris. It is a highly dangerous practice, as the amount of irritation thus set up is very apt to give rise to suppurative inflammation of the iris and its consequences. The use of the scissors is attended with far less risk, and better results.

These jottings are the results of experience and observation, and, I trust, may prove serviceable in leading to a correct use of these valuable but often misapplied remedies.—*London Medical Press and Circular*.

2. *On the Anæsthetic and Sedative Properties of Bichloride of Carbon, or Chloro-Carbon.* By J. Y. SIMPSON, M.D.

The bichloride of carbon, or chloro-carbon, is a transparent, colorless fluid, having an ethereal and sweetish odor, not unlike chloroform. Its specific gravity is great, being as high as 1.56, whilst chloroform is 1.49. It boils at 170° Fahrenheit, the boiling point of chloroform

being 141° . The density of its vapor is 5.33, that of chloroform being 4.2. Besides trying the anæsthetic effects of bichloride of carbon upon myself and others, I have used it in one or two cases of midwifery and surgery. Its primary effects are very analogous to those of chloroform, but it takes a longer time to produce the same degree of anæsthesia, and generally a longer time to recover from it. Some experiments with it upon mice and rabbits have shown this—two corresponding animals in these experiments being simultaneously exposed, under exactly similar circumstances, to the same doses of chloroform and chloro-carbon. But the depressing influence of chloro-carbon upon the heart is greater than that of chloroform; and consequently, I believe it to be far more dangerous to employ as a general anæsthetic agent. In a case of midwifery in which it was exhibited by my friend and assistant, Dr. Black, and myself, for above an hour with the usual anæsthetic effects, the pulse latterly became extremely feeble and weak. In another case in which it was exhibited by Dr. Black, the patient, who had taken chloroform several times before, was unaware that the new anæsthetic was different from the old; the pulse continued steady and firm, although she is the subject of valvular disease of the heart. The surgical operations in which I have used chloro-carbon have been, the closure of a vesico-vaginal fistula, the division of the cervix uteri, the enlargement of the orifice of the vagina, and the application of potassa fusa to a large flat nævus upon the chest of a young infant. In all of these cases it answered quite well as an anæsthetic. The child did not waken up for more than an hour and a half after the employment of the caustic, which was used so as to produce a large slough. Its pulse was rapid and weak during the greatest degree of anæsthetic sleep. One of the mice exposed to its influence, and which was removed from the tumbler where the experiment upon it was made as soon as the animal fell over, breathed imperfectly for some time after being laid on the table, and then died.

Chloro-carbon, when applied externally to the skin, acts much less as a stimulant and irritant than chloroform, and will hence, I believe, in all likelihood, be found of use as a local anæsthetic in the composition of sedative liniments.

In two cases of severe hystericalgia I have injected air loaded with the vapors of chloro-carbon in the vagina. The simplest apparatus for this purpose consists of a common enema syringe, with the nozzle introduced into the vagina, and the other extremity of the apparatus laced an inch or more down into the interior of a four-ounce phial, containing a small quantity—as an ounce or so—of the fluid whose vapor it is wished to inject through the syringe. Both patients were at once temporarily relieved from the pain. The first patient told me her relief at the first application of the anæsthetic vapor was so long that she slept during the following night far more soundly than she had done for weeks previously.

The injection of the vapor of chloro-carbon into the rectum does not prove so irritating as the vapor of chloroform. In one case it removed speedily pains in the abdomen and back.

Chloroform vapor applied by sprinkling a few drops on the hand, and held near the eye, is one of the very best and most sedative collyria in some forms of conjunctivitis, ulceration of the cornea, with photophobia, &c. I have not yet tried the vapor of chloro-carbon, but perhaps it may answer still better, as less irritant, and almost as strongly sedative.

I have found ten or twenty drops injected subcutaneously by Dr.

Wood's syringe repeatedly relieve local pains of the walls of the chest, abdomen, &c., without being followed by the distressing nausea so frequently the result of the hypodermic injection of preparations of opium and morphia.

Internally I have only hitherto tried it in small doses in gastrodynia, where it has the same effect as swallowing a capsule of chloroform.

The specimen of chloro-carbon which I have used was made by Mr. Ransford, who sent it down to Messrs. Duncan, Flockhart, & Co., of Edinburgh, under the idea that, by a chemical substitution, it might be converted into chloroform, and make a cheap medium for the manufacture of the latter drug. And perhaps I may be permitted here to remark that the quantity of chloroform used is now becoming very great, and might possibly be rendered greater if it could be produced at a still cheaper rate. We have two or three manufactories for chloroform in this city. The chief of these manufactories for it—that of Messrs. Duncan, Flockhart, & Company—now make upwards of 7,000 doses of chloroform every day, counting two drachms as a full dose; they thus send out nearly 2,500,000 doses a year. Are every two million and a half full doses which are used of opium, antimony, aloes, epsom salts, &c., attended with as little danger and as few ultimate deaths as these annual 2,500,000 doses of chloroform?—*Medical Times and Gazette*.

EDITORIAL.

MEDICAL DEPARTMENT OF THE ARMY.—We have neither the space nor the desire to enter into any extended examination of that portion of Senator Wilson's bill for the reorganization of the army which concerns the Medical Department; but there are two points that we feel it our duty to notice, as well as to enter, as we believe, an unavailing protest—for it is understood this section of the bill will pass as reported, it having undergone, in the Military Committee, the proper amount of manipulation by the Surgeon-General's office, where the good old rule sufficeth,

“That they should take who have the power,
And they should keep who can.”

We have no hope that the public will ever adequately recognize the value of the services of the medical staff of the army, or that Congress will cease to manifest indifference, and even impatience, when called on to legislate in its behalf. The medical officers of our army have always had to contend against the prejudices and intolerance of the officers of the line, and other staff departments. They were “doctors,” and nothing more; any pretension to be military officers was to be rebuked and checked. Whilst they might command enlisted men by virtue of their commission, they in their turn could be commanded by a brevet second lieutenant. We have heard an officer of the old army, who

during the rebellion was on "bomb proof duty," and never heard the whistle of a hostile bullet, boast, that when a fledgeling from the Military Academy, with the rank of second lieutenant, and temporarily in command of a post on the frontier, he had threatened to put one of the senior surgeons of the army, with the rank of major, in irons, if he left the post without his permission. This injustice and narrow-mindedness are born of the notion that the "doctor" is a non-combatant*—a tolerated civilian in the army organization, with only nominal rank. This prejudice against the medical officer is not peculiar to our service—it flourishes to the same degree in the British, French, Russian, and German. It prevails with greater intensity in the navy, and the naval medical officer often suffers gross insults from the quarter-deck autocrat. This anomalous and unfortunate position of the army surgeon, and which exposes him to frequent indignities, is due simply to the absence of substantive rank, which it is not desired should confer any military command outside the hospital or beyond the specific sphere of duty—for no medical officer would contend for an authority he is not qualified for. A step was taken, about a year ago, by the War Department in the right direction, by which the status of the medical army officer was materially improved, and the conferring of brevet rank was another mark of actual progress, and went far towards assuring to him a respectful recognition of rightful position. We had hoped to see this action of the Department sustained by legislative enactment; but we have looked in vain for any such provision in the present bill. Even the old grades remain the same. Tradition and fossilism have triumphed, and the medical officer, after serving forty years, finds himself no further advanced in rank than he might have been thirty years before. A majority, to which he is entitled after five years of service, is the limit he may reach. He sees all his brother officers, who entered the service with him, promoted, as they advance in years. But he is met with the inexorable stay-law of "thus far shall thou go, and no farther." For him to mount upwards becomes as impossible as for a frog to bound through a flag-stone. The evils of such a system are too palpable to need remark, and yet there is every reason to believe that this injustice, and, as we know, great injury to the service, are to be perpetuated in the new army. The argument "it has been," therefore "it ought to be," has won the day. Deprived of substantive rank, debarred from promotion, after arduous

* During the late rebellion, thirty-six medical officers were either killed or died from wounds received in action.

and dangerous service, and its welcome and needed attendant, increase of pay—an ever-present sense of imposed inferiority, keenly felt, with

“no spur

“To prick the sides of their intent”—

is it to be wondered at that medical youth of conscious ability hesitate before entering the army, or, at the period when matured experience makes them useful to the country, have to resign, and seek in civil life more solid rewards? A surgeon, after ten years of active service in that grade, should have the rank of Lieutenant-Colonel, and after fifteen years, the rank of Colonel.

A general order was issued by the War Department, dated October 31st, 1865, by which all the officers created by the Act approved April 18th, 1862, for the reorganization of the Medical Department of the Army, with the exception of the Surgeon-General, were disbanded—the appointments from civil life were mustered out of service, and the surgeons of the old army returned to their former position in the medical staff. This was done, as expressly stated in the order, on the recommendation of the Surgeon-General; the alleged plea was, their services being no longer needed. Had the Assistant Surgeon-General, the Medical Inspector-General, and the sixteen Medical Inspectors become offensive to certain “startled hopes,” and was it necessary to call in the willing and friendly hand of power to brush them away and “tread upon ’em?”

“He that stands upon a slippery place,

“Makes nice of no hold to stay him up.”

The section of the new bill, as introduced by Mr. Wilson, provided for five Inspectors in the new organization of the Medical Department, to be appointed in the usual way; but the bill, as reported by the Military Committee, makes these Inspectors, as well as one Medical Purveyor with the rank of Colonel, and four Assistant Medical Purveyors with the rank of Lieutenant-Colonel—in all, ten officers of rank—details to be made by the Surgeon-General, and during his pleasure: thus giving to the chief of the Medical Bureau a degree of patronage exercised by no other head of a staff department in the army, and which, no matter how wisely used, must give rise to more or less ill feeling, and tend to cause heart-burnings, and discontents, and jealousies. It puts an amount of power in the hands of the Surgeon-General which, used to reward docility or punish independence, is in direct variance of all precedent, and full of danger to the service. We do not understand the need for an Assistant Surgeon-General. A chief of the Purveying Department is necessary, and he should have

rank commensurate with his responsibility. Why increased rank should be given to Assistant Medical Purveyors we are at a loss to guess. This duty should not be imposed on medical officers at all. Trained business men should be appointed as medical storekeepers, to take charge of and distribute the supplies. We would have three subdivisions—Medical and Surgical, Sanitary, and Statistical—in the Surgeon-General's office, with a competent chief in charge of each division, who should work the routine details of his division, and they together should form a consultative council to assist the Surgeon-General on the subjects coming within their respective branches, the Surgeon-General being the sole responsible and administrative head. There should be an adequate number of Medical Inspectors, attached to the Medical Bureau, and detailed for special service from thence to make frequent visits of inspection throughout the different Military Divisions, consulting with the chief medical officers on all measures for the prevention and mitigation of disease, and making reports direct to the Surgeon-General on the sanitary condition of troops in garrison, stations, camps, barracks, hospitals, and attached to armies in the field in time of war, to assist and advise with the chief medical officer. For obvious reasons Medical Directors of Departments, Army Corps, and Armies, should have temporary or local rank, as provided in the Act approved February 23, 1865.

THE LATE BVT. LIEUT.-COL. RICHARD H. COOLIDGE, SURGEON U. S. ARMY.*—As we are going to press the sad intelligence comes to us that Bvt. Lieut.-Colonel RICHARD H. COOLIDGE, Surgeon U. S. Army, died at Raleigh, North Carolina, on the 22d of January. We have learned no particulars in connection with the melancholy event.

In the death of Dr. COOLIDGE the Medical Department of the Army has lost a shining ornament. As an officer he was surpassed by none in his corps for culture, intelligence, industry, conscience, and the best moral attributes. He had a clear head, excellent judgment, quick perception, and though his mental character was essentially reflective, it was also fairly receptive, liberal, and progressive. He was a discreet and sagacious reformer, and an honest, steady, but not boisterous champion of the rights of the army medical officer. Many of the ameliorations in the condition of the army surgeon which have been gained within a few years originated with him, as the records of the Sur-

* Entered the Army as Assistant Surgeon, 1841; promoted Surgeon, 1860; appointed Medical Inspector, 1862; disbanded, and reduced to Surgeon, October 31, 1865; assigned as Medical Director, Department of North Carolina, November, 1865.

geon-General's office will bear witness. His valuable and laborious contributions to Army Medical Statistics are known and appreciated. He had pride and faith in his profession, and kept apace with its literature. In both official and social intercourse, Dr. COOLIDGE showed a high sense of honor, native courtesy, and a kindly and genial disposition. His manners were quiet and engaging. In conversation he was a good listener, as well as a good talker. His amiable qualities and social instincts made him a general favorite wherever he chanced to be, as well without as within the professional circle. In Philadelphia, where he was stationed for a period of two years before his transference to the Department of North Carolina, he secured, by his modest but solid worth, troops of friends, who, regretfully parting with him, now mourn the loss of him they shall know no more. In naming his personal and moral qualities, let it not be forgotten that he traveled on the common way "in cheerful godliness," his piety being earnest, true, and simple, and he had a full comprehension of the humanities of religion.

Thus has died an accomplished physician, an exemplary officer, a worthy gentleman, a faithful Christian. And though "his sun went down while it was yet day," life to him had been no empty dream, but an ever-present reality, for his part in it had been pleasing and well acted, and those whom he loved so well may be comforted in their great grief by the thought, that while they have "lost this friend out of sight," the store of Paradise has gained.

PHOTOGRAPHIC LINE ENGRAVING ON STEEL.—We have recently seen specimens of morbid anatomy, geology, and natural history, almost instantly engraved by this process on steel, securing plates which will give any number of copies of the object required, with all the delicacy and minuteness of detail of photography, and the force and transparency of line engraving. We have good reason to think that difficulties which, but a short time ago were looked upon as insurmountable, have been conquered, and that photographic line engraving on steel has become a practical and reliable art. The process is accomplished by two means—optical and chemical. A photograph is taken from nature, and by optical and chemical means converted into a line engraving. Former essays to preserve photography on metal have been limited to aquatinto—a variety of engraving defective in transparency and incapable of producing any large number of prints. To Baron Egloffstein, well known in connection with the topographical surveys of the Pacific coast, and who served with distinction in our army during the first years of the war, belongs the credit of the discovery and application of this art, from which we look for valuable results in photomicrography.

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MARCH, 1866.

ORIGINAL COMMUNICATIONS.

Paralysis from Peripheral Irritation, with Reports of Cases.

By S. WEIR MITCHELL, M.D., Philadelphia.

[Continued from p. 355.]

PART SECOND.—*Gun-shot wounds giving rise to paralyses of remote portions of the body. A theory of shock from physical injuries.*

The following, and concluding portion of my paper, will be chiefly made up of the language and cases already made use of in Circular No. 6, for which I am jointly responsible with my former colleagues, Drs. Morehouse and Keen.

Together with these gentlemen, I enjoyed the privilege of studying hundreds of cases of gun-shot and other injuries of nerves in the U. S. A. Hospital, set aside for these cases by the order of Dr. Hammond, late Surgeon-General.

The wisdom of the policy which founded special hospitals in Philadelphia was amply justified by the medical results, while without such a classification of disease and injury as these hospitals effected, it was scarcely possible to look for any careful scientific study of rare instances of disease or wounds.

In the original manuscript of our paper on reflex paralysis, Dr. Hammond was credited with having established the Hospital for Diseases of the Nervous System. His name was, how-

ever, omitted in the published copy, which was distributed by the U. S. A. Medical Bureau. I have, therefore, the more pleasure in rendering this late act of justice to this distinguished physician.

In making the following quotations, I have very rarely altered the language employed; I make here this explanation, because nearly throughout we employed the term, Reflex Paralysis. It was so used as expressing clearly enough to most physicians what we meant, and not because we felt confident as to the theory on which this nomenclature was based, and which, indeed, we criticized and rejected at the close of our report of cases.*

The text of the original circular, so often alluded to, will hereafter be inclosed in parentheses, to distinguish it from my own additions.

(So far as we are aware, the medical histories, which we are about to record, stand alone as the first reports of sudden reflex paralysis from mechanical injuries. How they differ from the paralytic affections which result from disease of organs, and which have been so ably treated of by numerous authors, we shall presently consider. That they have thus far escaped notice may be easily accounted for. In the first place, they are rare; among some two hundred or more of carefully studied instances of wounds of nerves, we have met with only seven cases of reflex paralysis of remote organs, in which the influence was prolonged or severe. In the majority of cases, the effect was either very slight or very transient, and for one or both of these reasons unlikely to attract notice from surgeons on the battle-field, or in division or corps hospitals, where their brains and hands are taxed to the utmost by the palpable misery of wounds in the early stages of treatment. Had it been otherwise, we do not doubt that numerous cases of reflex paralysis from injuries to nerves would have been recorded.

The various effects produced upon the nervous system by gun-shot wounds have received, it seems to us, far less attention and far less study than their interest and importance appear to call for. Among them are some which must clearly be classified with the rare results illustrated by the cases re-

* Med. Circ. No. 6, 1864, p. 15 et seq.

ported in this paper; but there are also others which are far more numerous, in fact very common, and which are signally exemplified on every battle-field.

These have been more or less vaguely treated of as shock, commotion, stupor, etc. The larger part of those who receive flesh wounds involving no important organ are but little affected at the time, or may even be unconscious of having been hit, and exhibit no well marked immediate constitutional disturbance.

In other cases, and particularly in wounds of graver nature, the patient instantly falls senseless, and so remains during a few minutes, or many hours, when he revives again, either completely, or to suffer from a continued state of depression known as the shock, and marked by the usual features of great weakness, feeble circulation, pallor, etc.

In other cases the last named symptoms come on at once, and without the intervention or accompaniment of unconsciousness.

These very interesting states of system may be due, it seems to us, either to an arrest or enfeeblement of the heart's action through the mediation of the medulla oblongata and the pneumogastric nerves, or to a general functional paralysis of the nerve centres, both spinal and cerebral, or, finally, to a combination of both causes.

Arrest of the heart movements is producible, as is now well known, by any violent irritant directly addressed to the trunks of the pneumogastric nerves, or to the medulla oblongata, and it is conceivable that such an effect may be brought about by any very severe injury of an external part.

In fact, it has long been known that the sudden crushing of a limb in inferior animals will stop the heart, or make it act slowly, for a greater or less length of time. Now, if we add to this M. Bernard's experiments, in which he showed that irritation of the posterior roots of spinal nerves suddenly checks the cardiac motions for a time, and that like irritation of the anterior or motor roots gives rise to no such result, we shall be able to see how it is possible that a gun-shot wound of a large limb may be competent to occasion a like effect. We should remember, too, that in nearly all of these cases the hemorrhage from large vessels, such as are usually opened by accidents of

this nature, is sufficient, even during syncope, to add to or deepen, so to speak, the effects of the reflected nerve impression. Where small vessels only have been wounded this might not occur; but it is proper to state that men who have fallen senseless at the instant of the wound, frequently awaken, after a time, to find themselves drenched with blood.

Supposing such an arrest of the heart movements to have taken place, a continuance of their stoppage, even for a brief period, would naturally give rise to cerebral anæmia, pallor, unconsciousness, and the remaining phenomena of shock.

Again, as we have said, a severe injury, as a gun-shot wound of a limb or the neck, may produce its effects of unconsciousness and loss of power, by greatly weakening or for a time destroying, with various degrees of completeness, the functions of all the nerve centres, and of their conducting cords.

The influence of shock in causing temporary paralysis of nerve trunks is very well known to every experimenting biologist. Thus, after opening the spinal cavity, it is very common to discover that the sensitive nerves are, for a time, unimpressible by irritants. But, as a general thing, this is not so as regards the nerve centres within the skull, which are rarely so disturbed by the operation of uncovering them as to refuse all reply to irritations.

The majority of physicians will no doubt be disposed to attribute the chief share in the phenomena of shock, in its various forms, to the indirect influence exerted upon and through the heart. There are, however, certain facts which, duly considered, will, we think, lead us to suppose that in many cases the phenomena in question may be due to a temporary paralysis of the whole range of nerve centres, and that among these phenomena the cardiac feebleness may play a large part, and be itself induced by the state of the regulating nerve centres of the great circulatory organ.

The loss of consciousness, and the appearance of a state outwardly resembling syncope, prevent us in most cases from feeling sure that the great nerve centres suffer loss of function primarily, and not through want of nutrition from feeble or arrested heart action. But there do exist certain cases, more rare, it is true, in which singular affections of the nerve

centres, other than those of the heart, occur as a consequence of wounds.

These are well described by Legouest, in his *Surgery of the Crimean War*, p. 219. In somewhat varied shapes they must have been seen by all who are familiar with the early history of gun-shot wounds.

The patients to whom we refer do not fall when struck, but become insanely excited or almost hysterical. The author above alluded to supposes that this form of nervous excitement occurs chiefly among those who are already stimulated by immediate conflict, and who are actively engaged.

One well known instance has been related to one of the authors of this paper, as having taken place in the Mexican war, in 1846. An officer was wounded in the heel, and was thrown at once into a state of alarm, which caused him to exhibit signs of the utmost trepidation. His character for courage was such that the favorable verdict of a subsequent court of inquiry was scarcely needed.)

In the following case there was primarily excitement of brain, and finally general loss of power.

Col. P., æt. 41, P. V. In good health previous to date of wound; June 3, 1864, while standing behind the line of his regiment, which was not then actively engaged, he received a large slug in the right wrist joint. When struck he whirled half around, and then, with a sense of confusion and bewilderment, began to run along the back of the line with some vague intention of reaching a hospital. After running about fifty yards he fell insensible. He describes himself as feeling "excited, half crazed." He revived within a quarter of an hour, and soon after, in fact within one hour, suffered amputation of the forearm. His after history is most curious, as he became the subject of violent and extensive choreal movements of the stump and arm, on account of which he consulted me. Notwithstanding this annoying malady, he led his regiment through Sheridan's valley campaign and the final battles in front of Richmond.

I should be very glad if surgeons who are aware of cases like this one, or who are cognizant of any of the less common phenomena of shock, were to put them on record. Having had

no experience in the field, I have been obliged to limit myself to such cases as have accidentally fallen in my way. No doubt others, far better illustrating my views, are within the experience of those who have actually served under fire.

(We are tempted to add the following case as a still better proof that cerebral disturbance, the result of a shock other than traumatic,* may give rise to the most profound prostration.

A well known apothecary in Philadelphia was making a mixture of certain medicines in a large mortar, when they exploded with such violence as to break the windows of the store, crack bottles and jars, and deeply indent the board on which stood the mortar. Both he and his assistant were thrown down. Both suffered rupture of the tympanal membrane of the right and left ears. The assistant felt no marked constitutional effects, and got well with good hearing, but with a constant buzzing in the left ear. The apothecary himself, of a more nervous temperament, and perhaps, also, because he was standing nearer to the point of explosion, was, he thinks, insensible for a moment. When seen by one of us, a few minutes later, he was lying on a bed, with a pulse of rather remarkable strength, but now and then losing a pulsation, and altogether irregular as to rhythm, beating one minute seventy and eighty the next. His manner was excited and hysterical. He talked incessantly, and his limbs were in continual agitation, with occasional twitching of the facial muscles. The tympanal membrane was split across in his left ear, and on the right side presented a triangular opening. He had no headache, but complained of roaring, hissing, etc., which seemed to be sources of the utmost annoyance. Despite his desire to move about, his muscles were extremely feeble, and for twenty-four hours he was unable to walk without aid. He recovered readily, both membranes healing completely, and his hearing being none the worse for the accident.

The patient, it should be noted, was not injured by his fall, and as we have seen showed no signs of concussion. He, as

* In fact, there was traumatic injury, rupture of both tympanal membranes, but the effect in the way of violent stimulation of the nerve of hearing, in all probability, overbalanced the influence of the wounds, and gave occasion to the symptoms which arose.

well as his medical attendant, attributed the phenomena which he exhibited to the shock given to the auditory nerve. This opinion has since been confirmed by the cases reported by M. Brown-Séquard and others.

Further on we shall show that in rare cases gun-shot wounds cause partial or very general paralysis of grave type and prolonged duration in parts not directly injured; we shall also show that these protracted paralyses must be due to an equally permanent affection of the nerve centres. Now, if this be possible, there is every reason to believe that a temporary, though general, paralysis may take place in a large number of gun-shot wounds.)

I shall now proceed to relate instances of gun-shot injury which were immediately productive of paralysis of parts more or less remote from the seat of the wound.

(CASE I.—*Ball wound of right side of the neck, probably involving no important nerve directly; fracture of hyoid bone; wound of throat. Reflex paralysis of left arm; probable reflex paralysis of right arm; early recovery of left arm; partial and remote recovery of right arm.*

Captain R. N. Stemple, U. S. N., æt. 49. While commanding the ram Cincinnati, May 10, 1862, the ship was attacked by two rebel rams. Captain S. was aiming a pistol when a ball entered his right neck, broke the hyoid bone, and traversing the neck emerged three and a half inches from the middle line, above and to the right of the superior angle of the scapula, through the edge of the trapezius muscle. He fell, half conscious and confused, but soon reviving, felt that *both* arms were paralyzed. His first impression was that he was shot through both arms. He was carried below in great pain, and spitting blood freely. The pain in the arms was made worse by movement and by passive motion. Pressure gave pain in the right arm and shoulder only, and in the right chest. Sensation was never entirely absent from either arm, but was dull in both.

His medical attendant, Dr. Judkins, of Cincinnati, who took charge of his case on the 19th of May, 1862, writes as follows: "When first seen by me the anterior wound was discharging mucus and pus, with saliva. His voice was hoarse; deglutition, which returned in part on the third day, was still difficult and

painful. He experienced severe pain in the supra-hyoidean group of muscles, and in the pharynx. His left arm was still slightly paralyzed, having rapidly improved. On the right side the deltoid, biceps, triceps, and brachialis anticus were completely paralyzed, and up to the date of this account, July 9, 1862, have improved very little. The muscles of the right forearm are nearly as much paralyzed as those of the arm, and the sensibility of the right arm has become painfully acute. Captain S. seems also to have lost, to a great degree, the use of most of the shoulder muscles on the right side."

The left arm was nearly well in four weeks, the sensibility and movements of both improving equally, so that now, July 18, 1863, he has no loss of function in the member, except slight want of tactile sensation in the ultimate distribution of the ulnar nerve. The right arm was but little better at this date, but the forearm had acquired every movement except supination, which seemed to be limited about one half, not by paralysis, but by contraction of the opponent group of pronating muscles. At this period sensation was entire in the right limb, but there was soreness, on pressure, in all the anterior arm muscles, and neuralgia in the arm and forearm. The nutrition of both arms was good, but the right was the smaller, measuring at the biceps $9\frac{3}{4}$ inches, while the left measured $10\frac{3}{4}$ inches.

During the slow progress of his case, Captain Stemple lost several small pieces of the hyoid bone, and although hoarse for many months, has recovered his voice, without serious change in its tone or power.

His convalescence, interrupted by many accidents, and by an attack of pneumonia, continued, up to the summer of 1863, and was largely due to the skill and care of his able medical attendant, Dr. Judkins. When placed under treatment, in Philadelphia, by Dr. Mitchell, July 18, 1863, Captain S. was still suffering from constant pain in the right arm. The left was well except as to the trifling loss of sensation mentioned above. The right forearm, though weak, possessed every movement except supination, as did also the hand, but the arm hung at the side useless, because there was scarce any abducting power and very little flexion at the elbow, both the biceps and long supinator being greatly enfeebled, and the former muscle,

as well as the brachialis anticus, almost entirely atrophied and lost to view. Excepting the trapezius and rhomboid muscles, all the shoulder group was nearly useless and was partially wasted.

From July 18 to September 9 he was faradized, at first every day, and then every third day—active and passive movements, which had already been employed, being of course continued. The result was a steady gain, ending in a cure as complete as could be hoped for in a case so severe. He regained every lost motion, and could raise his hand to his chin and abduct the arm about fifty degrees. The pronators alone remained intractable, despite every effort, but as the supinators and biceps developed themselves largely, even pronation gained somewhat, though not to such an extent as the other movements. The pain and hyperæsthesia diminished, but the former still exists. It is believed that a second course of similar treatment, about to be instituted, will further amend this case, in which all other means have utterly failed.

Dr. Judkins is of opinion that the entire paralysis was due to reflected irritation. We incline to this belief for anatomical reasons; but even though we admit that the paralysis of the right arm may have been caused by commotion of the brachial plexus, it is impossible to suppose that the loss of power in the left member could have been similarly produced. The only permanent lesion on that side was the loss of feeling on the ulnar side of the palm and fourth finger. With this exception, it regained its normal functions within three or four weeks. Whatever may have been the cause of injury to the right arm, it appears to have involved, more or less, nearly all the strands of the plexus, which is unusual in cases of traumatic injury from a ball. Its results were also more lasting than in the other arm. A year and two months after the accident, the right arm was a useless member. Faradization of the muscles affected restored their power very rapidly, so that the patient regained every movement of the limb, which is still improving—electricity having been temporarily laid aside in September, 1863.*

* This gentleman continued to improve up to October, 1865, when he passed from under my care, having taken command of a ship and gone to sea.

CASE II.—*Flesh wound of right thigh, without wound of any large nerve; complete paralysis of all four limbs; speedy recovery of the left arm, tardy recovery of the other limbs; subsequent analgesia of the right side.*

Jacob Demmuth, æt. 21, Swiss, enlisted July, 1861, Company "D," 108th New York Volunteers, a man somewhat below the average standard of height, of lymphatic temperament, and moderate intelligence. Reports himself as healthy up to the date of his wound, which took place at Fredericksburg, December 13, 1862. He was marching at double-quick when a fragment of shell, as large as a musket ball, struck his right thigh, at the junction of the upper and middle thirds, directly over the femoral artery. The fragment did not enter deeply, but merely lodged in the leg, and was removed a day later without injury to the vessel.

Effect of wound.—He fell, half conscious, and although aware that he was wounded, he could not fix on the site of the injury until he had examined the limb. He felt instantly a burning pain in both feet, in front of the right chest and in the right arm, and in the right thigh about the wound. At first he was entirely powerless, but after a few minutes the power of the left arm returned, leaving him paralyzed as to motion in the right arm and in both legs. He lay on the field twenty-four hours, the weather being very cold.

Sensation was defective in all the parts paralyzed as to motion. He had no pain in the back, but the burning pains alluded to above continued for a long time, and were always eased by cold applied to the wound. While the wound was healing he had headache and difficult, painful micturition. The wound closed in four weeks. During this period he regained the power to move the right arm, feebly and slowly, although perfectly as to extent. The pain in the side and feet also diminished, and the former disappeared altogether at a later period. He could not stand, however, or lift his legs from the bed at the time the wound healed, but there was then no headache or difficulty with the bladder or rectum.

January 28, 1863, he was sent to Washington, where he improved so as to be able to walk with the help of a cane. His subsequent transfer to West Philadelphia caused a relapse; the

pains returned, the paralysis increased, and he walked with difficulty on crutches.

June 4, entered Christian Street Hospital.

Present state. Movement.—The patient is partially paraplegic. He has some power to move the thighs when lying down, but can not lift the legs from the bed. Below the knee all motion is lost, except a slight power of flexing the smaller toes in both feet. Pressure upon the cicatrix causes feeble twitching of the anterior muscles of the right thigh; both legs are subject to cramp and twitchings, which increase at night. The left arm is strong; the right arm has all the normal movements, but all are slowly and feebly executed.

Sensation.—He has shooting pains which start from the seat of the wound, and dart down the thigh to the knee. No other pain exists at present, but there is still a good deal of burning sensation in both feet alike. Localizing sensibility perfect everywhere. Tactile sensation normal, or very nearly so, in all parts of his body. No loss of sense of pain in the skin. Pressure or pinching of the muscles gives him more than the usual pain, so that the muscles (of both legs, especially below the knees) may be regarded as affected with hyperæsthesia of common sensation. The left arm is in all respects normal; the right arm is also free from lesions of sensibility.

Nutrition.—There is no special atrophy of individual groups of muscles, but both legs are slightly wasted, the right arm not at all so. The legs below the knees are relaxed and cold, the feet are congested, but not swollen to any marked extent. Along the edges of both soles there are singular purple and blue mottled spots, which, he says, existed from the time his boots were first taken off, twenty-four hours after he was wounded. It is possible that these marks are due to frost-bite. The muscles of the legs are about equally irritable to induced electric currents. Unfortunately, no very perfect electric examination of their condition was made at this period.

Treatment.—Regarding the case as one of reflex paralysis chiefly, he was ordered to have rough frictions, with cold to the spine, and to take the twentieth of a grain of strychnia three

times a day. Under this treatment the cramps and twitchings increased, so that after three weeks the strychnia was abandoned. Every future attempt to repeat its employment caused the same increase of annoyance, without corresponding benefit, and it was finally laid aside as useless or worse. About the middle of August a blister was placed on the cicatrix, with the effect of greatly relieving the burning in both feet. At the same time he was ordered to use the hot and cold douche to the spine alternately, and to be faradized daily. The electricity was persistently employed during two months, and a month later he was also treated with iron and quinine, porter and liberal diet. The electric treatment caused a rapid amelioration of his case, so that he soon left his bed and began to walk on crutches. Early in November he ceased to improve, and the treatment was abandoned. At this time he could use his right arm well and quickly, and walked unaided, although with a little unsteadiness of gait.

No close examination was made as to his sensibility until December 3, 1863, because during this time he had been able to give aid in the wards, and made no complaint, except of more or less constant aching in the dorsal and lumbar regions of the spine. About December 3, he was closely inspected for discharge, when the following notes were taken:

Motion.—Good in left arm; not so perfect in right arm. Both legs somewhat weak, so that he shuffles a little in walking, the worst movement being that of extension in the toes of the right foot.

Sensation.—Tactile sensibility feeble in the right leg and right arm, but nowhere entirely lost; it is normal in the left leg and left arm. The sense of touch is first found to be feeble below the navel on the right side. It lessens in perfection to the knee, and is better below that part, especially on the inside of the calf, being worst in the foot. Irritating the sole causes no sensation of tickling on either side.

Pain.—There is absolute loss of sense of pain in the right leg, belly, chest, and arm, with somewhat lessened sensibility to pain on the left side also. In many localities he was able instantly to tell by the altered sensibility when the needle point crossed the median line; in others, this was more diffi-

cult. So complete was this analgesia, that the most intense faradization of the nails of the right hand, or of the right nipple, caused not the least sensation. The penis remained sensitive, but all over the right side of the body he could be cut or stuck full of needles without evincing the least consciousness of any thing but a touch.

The sense of temperature was good in the left leg, confused and uncertain at the upper third of the right thigh, and lost below the knee, where a heat of 110° Fahrenheit was felt as a touch only, when the sponge wetted with hot water was applied. On the foot of the right side this degree of heat was unfelt in any form. Higher heat caused reflex movements which did not tend to remove the limb from the irritant, but were merely convulsive in their character. Intense cold also gave rise to these irregular movements.

Electric Examination.—There was some difficulty in determining the state of the muscles as to their electric sensibility, owing chiefly to the want of intelligence in the patient, and to the fact that he spoke an impure German patois, which made it no easy task to obtain from him a clear statement of his feelings. The electro-muscular contractility was slightly diminished in the right leg and arm; it was much impaired in the extensors of the toes on both sides; everywhere the muscles responded slowly. The patient was discharged December 14, 1863.)

This case is one of unusual interest, and as the patient spoke English badly, we were at great pains to ascertain that we understood him perfectly, and that his case was correctly reported. Repeated examination, and the evidence of a comrade who belonged to his regiment, enabled us fully to satisfy ourselves that the history of his case, as he gave it, was perfectly truthful.

It began with an extensive paralysis from peripheral irritation. Strychnia disagreed with him remarkably, causing always an increase in the symptoms. This fact, and the later history, induces me to suppose that he had then begun to suffer from chronic myelitis, which developed itself more fully as his case proceeded.

(CASE III.—*Wound of right thigh, with probable commotion of right sciatic nerve; partial paralysis of right leg; reflex paralysis of right arm; speedy recovery of arm; history unfinished.*

William W. Armlin, æt. 23, born in New York, farmer, enlisted August, 1862, in Company "D," 134th New York Vols. Healthy before enlisting, and except a slight typhoid fever in the fall of 1862, healthy up to the date of the wound, July 1, 1863, at Gettysburg. While kneeling on the left knee, the right knee bent at a right angle, he was shot in the right thigh, on a line with the internal condyle of the femur, ten inches above it, and a little anterior to the artery. The ball passed upwards, backwards, and outwards, and emerged two inches below the tuber ischii, and one and a quarter inches external to it, just above the fold of the nates. Dropping his musket, he fell on his face, weak, but not insensible; the right leg violently flexed for a moment. He felt very feeble, but especially so in the right arm, with which he vainly tried to aid himself. After a half hour the bleeding, which was not excessive, ceased, and he was able to stand on the left leg, but not on the right leg, and had scarcely any use of the right arm, which, it should be noted, was in no way hurt when he fell.

He managed to bind up the wound with a water dressing, and occasionally renewing it, lay two days on the field. When hit, he perceived no pain, but within an hour a burning attacked his instep, and has never left it, remaining neither worse nor better. Sensation, he is sure, was unaltered, except on the sole; motion improved slowly, except in the flexors and extensors of the foot and toes. To his surprise, the feebleness of the right arm increased after he was put in bed, and indeed notably after the second day. Up to October 28 it improved slowly, but at this time he went home on furlough, and began to use a crutch, which again so weakened the arm as to alarm him, and deprive him, as at first had happened, of the power to feed himself. Rejecting a crutch on this side, he used a liniment on the arm, and it has now gained so much as to have about one-fourth the force of the left arm. It did not lose sensation at any time.

Present State, December 14, 1863.—General health good.

Nutrition.—Wounds healed. Leg below knee wasted, foot

swollen, toes blue. Contraction of great toe in flexion. Measurements: $8\frac{1}{4}$ inches above internal condyle the thigh measures, right, 16, left, $17\frac{3}{8}$ inches; middle calf, right, $11\frac{1}{4}$, left, 13 inches.

Voluntary motion.—He lifts the right thigh well, but complains of its weight. Knee motions very fair; has no extension or flexion of the foot or toes.

Sensation.—Tactility absent in sole of right foot, feeble in second toe on its dorsal face, absent on top of third toe, but elsewhere complete. Localization extremely confused, so that a touch on the toes is felt, but is referred to the instep. Surface analgesia of the sole, but deep pricking with a needle is felt in the sole. Hyperæsthesia of the posterior thigh muscles to a slight degree; marked soreness on pressure in the calf muscles, the short extensors of the foot and its whole dorsal surface, as well as the inside of the sole.

Pain.—The pain lies deep in the calf and extends outside, under and in the perineal muscles, down the front of the leg, and over the dorsum of the foot and to the external side. It is intense in the dorsum, but nearly absent in the sole. Water does not seem to ease the pain, which is of a burning character, "like mustard." Hitherto nothing has aided it.

Electric test.—The thigh muscles respond well. The right tibialis anticus has no electro-muscular contractility, but its electro-sensibility is highly exalted, as is the case in all the muscles down to the foot. In the foot the electro-muscular contractility and sensibility are both lost, except that in some parts of the dorsum the sensibility can not be tested readily for various reasons. It is certainly lessened. The right arm is still very feeble, especially below the elbow, and has lost in size. It measures, comparatively, as follows, Dec. 20th:

RIGHT ARM.	LEFT ARM.
Forearm, $8\frac{5}{8}$ inches.	$9\frac{1}{4}$ inches.
Arm, 9 "	$9\frac{3}{8}$ "

He is not left-handed. The arm is improving; the leg has remained unchanged during some time past.

Ordered—First, a blister over the whole dorsum pedis.

Dec. 23.—This has caused great relief to the burning pain, and is to be repeated.)

The hand and arm recovered easily and speedily under the use of faradization. The pain in the dorsum of the foot was several times relieved by blisters, and as often returned, being to some extent affected by the weather. It was not entirely well at the date of his discharge from service, in March, 1864.

The two cases which follow were observed in Filbert Street Hospital, Philadelphia. I regret that no fuller notes of them were taken, but of the facts as stated there can be no doubt.

(CASE IV.—A sergeant was shot, during the battle of Malvern Hill, in the right testicle. This organ was nearly entirely destroyed by the ball. He fell, without pain, believing himself wounded in the back. A few moments later he became senseless. Recovering after a few minutes, he discovered that he could walk, but that the right foot dragged when he attempted to lift it during the effort to get to the rear. This weakness remained permanent for several months, and was relieved by faradization and shampooing soon after the testicle healed. the flexors of the left foot were completely paralyzed to voluntary control, but responded to the irritation of the induced electro-magnetic currents.) There was no loss of sensibility. The paralysis gradually improved, but the scrotum healed before the power to lift the foot returned.

(CASE V.—*Shell wound of left thigh. Analgesia and Anæsthesia of a part of the right thigh symmetrically related to the seat of wound.*

An officer, Lieut. —, was struck by a small fragment of shell upon the external side of the left thigh. He felt pains of a smarting character in both thighs, at or about the same spot, and was impressed for a time with the conviction that he had been shot through both thighs. The shell wound healed in the course of three or four months. During this time he had occasional smarting on the outside of the sound thigh. This gradually disappeared, and at length he noticed, accidentally, that there was a space of skin about five inches square, on the outer part of the sound thigh, in which there was neither sense of touch nor of pain. When examined by us, without being allowed to see the part he could indicate the boundaries of the anæsthetic space very readily, by the loss of tactile sensations when a body, moved while in contact with his skin, was made to

cross the line on to the numb parts. These bounds were always very accurately the same. He returned to his regiment without any improvement having taken place in regard to the anæsthesia. The interest of the case just now recorded lies partly in the fact that, at the time of the wound, the patient felt a sensation which he referred to the part which afterwards became deprived of feeling.

The following case is instructive from its resemblance to Case No. 3, that of Armlin, in whom a gun-shot wound of the right leg also caused paralysis of motion in the arm of the same side.

CASE VI.—Gun-shot wound of right thigh; lesions of motion and sensation; reflex paralysis of right arm as to motion.

Daniel Kent, æt. 24; Pennsylvanian; farmer. Enlisted August, 1862, Company "B," 145th Pennsylvania Volunteers. Healthy until wounded. At Gettysburg, July 2d, 1863, while charging at a full run, the leg raised up, he was shot in the right thigh, $10\frac{1}{2}$ inches above the edge of the patella, directly over the rectus. The ball made its exit on the postero-internal surface of the thigh, one inch below the fold of the nates. It seems to have passed inside of the bone, and could not have hit the sciatic nerve. He fell at once, quite conscious, and feeling an instant stinging pain all over the right side of his body, and especially in the arm. He lost a great deal of blood, and found that he could not sit up without giddiness. His wound was dressed in six hours, and he was on the field thirty-six hours. The leg lost all motion and some sensation, and the tingling pain in the arm left him within twelve hours, the member remaining very feeble. He was in bed six weeks, and then was able to walk on crutches. The sense of touch changed but little during the time which has since elapsed, and the power of movement in the leg has remained unaltered since August 1st, 1863. The wound healed in October, with some previous loss of bone. Since October the wounds have twice reopened to give exit to small pieces of bone. Except an attack of ague in October, his general health has been good.

Present Condition, December 26, 1863. *Nutrition*.—The leg is healthy in color; the foot swells when hanging down. The

right thigh, eight inches above the patella, measures 19 inches in circumference; the left measures $19\frac{1}{4}$ inches. The right calf measures $14\frac{1}{2}$ inches; the left calf measures 15 inches.

Sensation.—No pain anywhere; tactile sensibility entire; sense of locality healthy.

Motion.—The thigh is voluntarily flexed very slightly, and only through the agency of the psoas muscle, the anterior thigh muscles refusing to obey the will; abduction and adduction of the thigh normal; extension of the thigh is normal; extension of leg, none. The foot is almost moveless, except that the will can cause feeble flexion of the toes, and slight eversion and inversion of the foot.

Electric examination.—The rectus muscle has its electro-muscular contractility somewhat lessened; that of the two vasti muscles is lost until the wet conductors reach the upper parts of the muscles, (three inches above the wound,) where this property becomes normal. The sartorius has its electric contractility diminished. Below the knee the peroneus longus responds very well; but with this exception, none of the leg muscles stir under the most powerful induced currents. The short extensor of the toes and the interossei still possess some power to contract under electrical stimulus. Throughout, the electro-muscular sensibility is diminished in all the muscles which have suffered in their contractile power, and the sense of pain seems also to be materially lessened, since dry electric conductors, with strong currents, cause no pain when applied over the bones or nails of the foot.

The history of the arm, which was reflectively paralyzed, has been reserved for separate detail here.

After three days from the date of the wound, the right arm, which had remained feeble, became so completely paralyzed that the patient could no longer raise it to his lips; under the use of a stimulating liniment it grew better until he used crutches. Probably owing to their employment he became much worse, but gradually improved again up to this present date of January 6, 1864. The right and left arms measure nearly the same; power of right arm one-fourth that of left.

Electric examination.—Electro-muscular contractility normal; electro-muscular sensibility somewhat lessened.

Treatment.—Faradization of arm daily; alternate hot and cold douche, and active motion.

On close examination, soon after admission, some evidence of tubercle was found in the right lung, and the patient was therefore ordered to be discharged, January 20, 1864.)

Case VII. of the circular rested almost entirely upon the statement of the patient himself. As it is not of much value, clinically, I have omitted it here.

The remaining case was not recorded in our original paper. It happened in the person of an officer with whom I am well acquainted, and whose case was referred to me for examination and treatment.

CASE VII.—Lieut. W. F. G., æt. 39, 16th U. S. I. Healthy up to date of wound. At Chickamauga, Sept. 19th, 1863, while lying down in front of a battery, his head towards the enemy, a conoidal ball struck his right thigh. The ball entered posteriorly, two inches below the gluteal fold, half an inch to the inside of the central line of the limb. It emerged anteriorly, four inches above the patella, one inch to the inside of the central line of the limb. He felt a general sense of shock, but no pain, and was only conscious that he was struck somewhere. He did not feel faint or lose consciousness. A few minutes later the battery ceased to fire, when he arose and saw the blood streaming down his leg and foot. He walked a little way and then fell, feeling faint and dizzy. The enemy's line passed over him and fell back again, when he was carried to our rear. The following day the ball was cut out, being immediately under the skin, and a water-dressing applied, and renewed until the wound healed, which was within about two months. At no time was there much pain in the wounded leg, unless it was handled. Even this ceased, soon after the extraction of a shred of clothing.

From the time he reached the hospital he was conscious of a feeling of intense cold in the left (unwounded) leg, and with this existed a distinct and very vivid impression that the wound was in the left leg. This impression was so strong that he mistook the unwounded leg for that which was hurt, when questioned by the first surgeon who saw him. This singular delusive sensation was strengthened by the more or less con-

stant pain in the left thigh, and by the general absence of this annoyance in the wounded part. He has been so often examined and so much questioned as to this feature of his case, that he is somewhat indisposed to own how decided and, as he says, how almost irresistible is the false reference of the site of injury.

The coldness to which I have referred was such as to make him think the part was frozen. A servant rubbed it at frequent intervals during the night which followed the battle, and such warmth was applied as could then be made use of. The next morning he observed that the left leg was bloodless and white, from the knee down. This condition improved and alternated with flushes of heat and redness of surface. The pallor and coldness disappeared in about three weeks, returning afterwards at intervals more and more remote until the early spring, when it was felt but very rarely.

The night after he was wounded he became aware that his right arm was so weak as to give way under him when he tried to move, and that the tactile sense was deficient in the right hand, to such an extent that he could not button or unbutton his garments. The double defect of sense and motion became very evident, when, a short time after he was shot, he attempted to write, but found that his pen constantly fell from his fingers, owing to its being unfelt. He thinks that like troubles existed, in a very much less degree, in the left hand. About the second week he began to have pain in the small of the back, and soreness in the præcordial region upon pressure or deep expiration. During the process of cicatrization he lost twenty-two pounds of his weight, and, since then, nine pounds more.

While the wound was healing, the superficial veins of the right leg enlarged considerably. By careful bandaging, this appearance, which was chiefly above the knee, was much lessened, but it was succeeded by similar and rather remarkable enlargement of the veins of the left thigh.

At the date of June 2, 1864, Lieut. G. reported at the U. S. A. Hospital for Injuries and Diseases of the Nervous System.

At this time he was a tall, thin man, looking prematurely old, very pale and feeble, and somewhat bent. He had twice attempted light office duty, and each time had fallen ill. His

general health was bad. He was so weak, as to be unable to walk more than two or three squares; had dyspepsia, bad appetite, was very costive, liable to attacks of dizziness on exertion or when stooping. No headache, but frequent pain in the small of the back, but no tenderness in any part of the spine, from pressure, cold, or heat. Had constant pain in the legs, chiefly in the left thigh, but more in the right than he formerly had. No urinary troubles. His *morale* was good. Occupied himself in reading, and, of necessity, spent most of his time on his back, since the least exertion on foot entirely prostrated him and increased the feebleness of his limbs. All his pains were worse in bad weather, and in the afternoons. He was still losing ground.

Lieut. G. remained under my care during the autumn of 1864, and up to February, 1865. During this interval he took constantly, thrice a day, pills containing a grain of lactate of iron, half a grain of extr. *ignat. amar.*, and a grain of quinine. After a month of this treatment I added to it six grains of quinine daily, and three ounces of whisky. Bandages were also applied to the legs, and finally an elastic stocking from the groins to the toes. Under the use of these means, with occasional diminution, for a week or two, in the amount of quinine, he slowly gained ground. On February 20th, 1865, although far from well, he had lost all trace of weakness in the arms, and was able to walk two or three miles without much fatigue. The pains in the legs also disappeared to a great degree, but the mental impression of the bullet wound, as existing in the left leg, still remained.

March 20th, 1865, I heard from Lieut. G., to the effect that he was still improving. His case has, I am informed, continued to progress favorably.

I should have added, that when examined in Cincinnati by a board of medical officers, their president, Dr. Head, U. S. A., regarded Mr. G.'s case as one of reflex paralysis.

The above stated cases were undoubted instances of paralysis from injury to some region remote from that whose function suffered loss. Two of the cases, Numbers I. and VII., have been under observation up to this present date. The other histories are more or less incomplete, the patients having been

discharged the service or otherwise removed from the wards. Within the limits reported, the cases were watchfully studied, not only by myself, but also by my able colleague, Dr. George Morehouse, and by our assistant, Dr. Keen, who resided in the hospital. I quote, with such corrections as were needed, the following summary from our original analyses of the cases already stated.

RELATION OF WOUND TO THE PART SECONDARILY AFFECTED.

(CASE I.—The wound involved the muscles of the neck or throat, and the hyoid bone. *Result.* Paralysis of both arms and of the neck.

CASE II.—Fragment of shell; wound of muscles over and external to the right femoral artery. The injury may have caused concussion of the crural nerves, and thus much of traumatic paralysis. *Result.* Paralysis of the right arm and leg, and the left leg.

CASE III.—Probable injury of the sciatic nerve, (commotion.) *Result.* Paralysis of the right arm.

CASE IV.—Ball wound of right testicle; paralysis of right anterior tibial muscles and peroneus longus.

CASE V.—Wound by fragment of shell in external side of left thigh; anæsthesia and analgesia on a corresponding part of right thigh.

CASE VI.—Ball wound, probably involving the crural nerves. *Result.* Paralysis of right arm.

In four of these cases the leg was hit, and the arm of the same side was paralyzed. In three cases the paralysis affected the opposite side of the body, and in one the paralysis of tact and pain was observed to have fallen upon a space symmetrically related to the wounded spot.

No general law, therefore, can be deduced from these records; nor, from what we see in the causation of reflex paralysis from disease, should we expect to find any inevitable relation between the part injured and the consequent paralysis.

The constitutional condition at the time of the wounding, as to excitement, mental and physical, may possibly have to do with causing the resultant paralysis.

Of the seven cases above reported, two were in active movement, two were standing, about taking aim, one was kneeling, one lying down, and of two we have no information as to this point. It may prove, upon examining a larger number of cases, that a man wounded when moving violently, or when excited, is more than another liable to reflex paralysis, but as yet we are not entitled to such an inference.

In most of our cases the constitutional effects were instant and severe, and could not therefore have been due to the loss of blood, which in some of them was copious. Four of the seven cases had stinging, smarting or burning pain in the part paralyzed. The pain was an early symptom, which disappeared in all of them after a time. In three cases no such pains were complained of.

The after-history of these cases is extremely curious. However grave the lesion of motion or sensation, it grew better early in the case, and continued to improve until the part had nearly recovered all its normal powers. In almost every instance some relic of the paralysis existed, even after eighteen months or more from the date of the wound. In some the part continued weak, in others a slight loss of sensibility remained, and in two the loss of power and of sensory appreciation was very considerable, even at a late period.)

I do not propose, at this time, to consider the subject of the treatment of these cases. So far as we could judge, faradization afforded good results. It was usually combined with good diet, tonics, and gentle use of stimulus—agents such as were sure to afford good results in men who had been long exposed to every hardship of warfare. In the case of Lieut. G. no electricity was employed; but the good effects of rest, with the use of tonics and stimulants, were very striking. It will be observed that the indications were derived rather from the general condition of our patients than from any theory entertained by us as to the peculiar condition of the centres which induced the palsy.

On Bony Growths in the Meatus Auditorius Externus. By D. B. ST. JOHN ROOSA, M.D., Clinical Lecturer on Aural Surgery and Ophthalmology in the University of the City of New York.

CASE I.—Mr. C., æt. 39, was seen in April, 1864, in consultation with Dr. C. R. Agnew, under whose care he had been for some time. He had lost, before coming under observation, the hearing of his right ear, by inflammation and caries of the middle and internal ear. Previous to the above date Dr. A. had removed a sequestrum, consisting of the cochlea and semicircular canals, from the depths of the external auditory canal of the ear, and thus terminated the inflammatory action.* In early life Mr. C. had also suffered from "inflammation" of the left ear, producing the bony growths in the external auditory canal which render his case the subject of present description. He now hears with this ear a watch tick at a distance of five inches. In the auditory canal, near the meatus, are two bony enlargements, which rise from the anterior and posterior walls, and project in a conical form, so as to occupy at least three-fifths of its caliber. These tumors have all the physical appearance of exostoses, and seem to have originated in periosteal inflammation. They have been steadily treated for many weeks by the local application of the saturated tincture of iodine, and certainly not diminished in size. Pressure upon them excites pain, and induces an increase of swelling in the skin which covers them, and thus temporarily adds to the deafness. The entire absence of hearing in the fellow ear, and the failure of simple means to render the exostoses smaller, have suggested the propriety of some surgical operation for their removal.

Such a proceeding has been thus far postponed by the occurrence of an acute attack of inflammation in the part, and extending to the tympanum, with symptoms of more than usual cerebral irritation. From this disagreeable complication he has entirely recovered, under Dr. Agnew's care.

His general health being impaired he went abroad, and while in London consulted Mr. Toynbee, who used bougies, hoping

* Tröltsch's Diseases of the Ear, p. 210.

to dilate the canal; but, according to Mr. C.'s statements, they caused much pain and accomplished nothing. Through Dr. Agnew's courtesy I again saw the patient in the spring of 1865, and found that the growths had so increased that only a small probe could be passed between them, and the hearing more impaired. The patient could still, however, hear the watch tick, but only when laid on the auricle.*

CASE II.—A gentleman, æt. 40, whom I saw but once, in June, 1864. He states that he had a "running" from his right ear for a number of years. For some two or three years past he had observed that the ear was stopped up. He was accustomed to remove the accumulating discharge by thrusting in a match armed with cotton. There is seen a bony growth arising from the posterior wall of the meatus, and involving the whole caliber of the canal, except a space large enough to admit an ordinary sized silver probe. Through this opening a slight amount of purulent discharge constantly makes its way. There was some hyperæmia of the pharynx, and there was a small ulcer on one of the tonsils. The patient was in excellent general health, was rather a free liver, and said he had constitutional syphilis, but no good evidence of its existence now existed. The patient had never had rheumatism or gout.

CASE III.—Mr. S., æt. 25, Conn., February 6, 1865, (a patient sent to me by Dr. Alfred North, of Waterbury.) When the patient was three or four years of age he had scarlet fever, at which time his ears began to discharge, and they have continued to do so at intervals ever since, with attacks of pain in the ears, which sometimes lasted for weeks, and prevented him from any occupation for the time. Eight years ago his ears were examined and polypi discovered, one of which was removed by caustics. The attacks of pain have continued to occur, the discharge continues, and his hearing is becoming more and

* Since the above article was written, and while it was in the printer's hands, the patient whose case is here given has died of inflammation of the membranes of the brain, induced by suppuration in the cavity of the tympanum, the pus not being able to find an outlet, on account of the presence of the exostoses. Dr. Agnew exhibited the brain and temporal bones before the New York Pathological Society, and proposes, at some future time, to present to the profession a complete history of this unique case.

more impaired. He is just now suffering from acute pain referred to the left ear. He hears the watch about one inch from each ear.

In the right meatus there is seen a bony growth reaching nearly out to the orifice of the external meatus, and arising from the posterior wall. The space between the growth and the anterior and upper wall is about large enough to admit of the introduction of a camel's hair brush. In the left meatus there is seen a gelatinous granulation, also reaching nearly out to the orifice of the meatus.

On blowing air into the cavity of the tympanum, by means of the eustachian catheter, air and fluid are heard making their exit into the external meatus, but the blocking up of this passage prevents their emergence. On the right sight pus may be seen in the orifice between the bony growth and the wall of the meatus.

The confinement of the fluid in the middle ear accounts for the pain in the left side, and the indication of treatment was to secure its free exit. This was done by removing the gelatinous growth by torsion, the patient being etherized, and rendering the eustachian tubes permeable by the use of the well known means, the catheter and Politzer's method. The granulation was found to have its origin from a general bony expansion of the meatus. This growth had no one point of attachment, but involved all the sides of the meatus, rather more expanded externally, giving the bony canal rather a funnel-shaped appearance. The bone was roughened. The pain in the ear disappeared as soon as these means for securing an outlet to the pus, constantly secreted from the cavity of the tympanum, and passing through the perforated membrana tympani, had been taken, and the hearing was so much improved that the watch was heard about four inches from the left auricle. He remained under treatment for a few days, and then returned to Waterbury, and has been under the careful and able observation of Dr. North, who has applied tincture of iodine to the exostosis of the right side, astringents of various kinds to the left meatus, the patient keeping the eustachian tubes permeable by means of gargles and Politzer's apparatus.

The last time I saw the patient was in October of this year,

(1865,) when the following note was made. "He has had no attack of pain in the ear since the first date. There is still a considerable discharge of pus from each ear. He hears ordinary conversation well, and the watch ten inches from his left ear, and two inches on the right—a gain of one inch and nine inches respectively." The bony growth on the right side has not increased any, and that on the left is now smooth and has a somewhat glistening appearance.

CASE IV.—Woman, æt. 27, at the N. Y. Eye and Ear Infirmary. No reliable history could be obtained from the patient as to her ears, except that she had been occasionally hard of hearing for some years. She was quite sure that she never had had a discharge from the ears; was in good general health, and had always been so. She could hear the watch two feet from the left auricle, and twelve inches from the right. The left membrana tympani showed evidences of previous inflammatory action, there being thickening of its mucous and fibrous layers. There is a bony enlargement of the posterior wall of the right meatus, so large as to prevent any view of the membrana tympani. The patient was seen but a few times, not continuing under treatment.

Remarks.—As has been indicated in the respective histories, these growths were rather general enlargements of the periosteum, and of the bone structure immediately beneath, than tumors—true exostoses. Their nature seemed to be inflammatory, or, rather, hypertrophic. Perhaps all the similar growths recorded in the literature of aural surgery are of this character, *i. e., morbid growths consequent on local irritation*—the irritating cause in these cases, with one exception, the last, being clearly ascertained to be the contact of pus passing from the middle ear. The process in its inception was probably a periostitis, which may exist independently of any dyscrasia. Mr. Toynbee details nine cases in his well known work on the Diseases of the Ear, and remarks that "they seem to be the result of a rheumatic or gouty diathesis." This certainly can not be said of the cases here given, and a careful examination of the histories of Mr. Toynbee's cases has caused considerable doubt, to the present writer, as to whether they, too, were not rather to be ascribed to local inflammatory action than to a diathesis.

Virchow's views as to the etiology of bony growths in general may here be given. "With respect to the etiology of the hyperplastic osteomata, the fact can not be lost sight of that *local* impressions were, in very many cases, the exciting cause. According to experience, entirely positive and generally very rude mechanical injuries form the ordinary starting point of the morbid process, and, as has been already shown, this process presents itself substantially as an irritative one, often even as inflammatory, so that a boundary between bony products of inflammation and osteomata can not generally be drawn."*

"Some have, indeed, educed the frequent cases where certain constitutional diseases, especially rheumatism, arthritis, syphilis, scorbutus, rachitis, have produced bony tumors, as being something opposed to these local causes. Undoubtedly the field of these conditions was formerly too widely extended, and we may say that scorbutus is now almost entirely excluded from the list of causes, and that the gouty enlargements of bone are no growths, (*gewächse*,) but only deposits, (*ablagerungen*.) But we may not deny the influence of the other so-called dyscrasia, especially of the rheumatic, syphilitic, and rachitic conditions. In spite of this, the influence must not be overestimated," etc.

"As to rheumatism and syphilis, we may not here even content ourselves with assigning constitutional causes, for the affection of one single bone must always be considered as dependent on a local impression."

As also interesting in considering this subject of bony growths, parts of an article by Professor Welcker, of Halle, are here reproduced.†

"Professor Seligmann has made the interesting statement that, in the various American skulls found in different collections, skulls known as Titicaians, Huankas, Aymaras, and which have been elongated by pressure during infancy, exostoses in the external auditory canal are very common. He says, of six skulls which I have, up to this time, examined, five have such exostoses. In the very similarly deformed so-called Avarian skulls, exostoses did not exist. This is certainly a

* Die Krankhaften Geschwülte. II. Band., I. Hälfte, p. 73 et seq., *passim*.

† Archiv für Ohrenheilkunde. I. Band., III. Hälfte, 1864.

remarkable phenomenon, and may well justify the inquiry, are these exostoses a peculiarity of race, or are they a certain production of an injurious cause, especially efficacious in this race? My honored friend, Professor Seligmann, has promised us a closer examination as to this. Still, I do not think that he will be able to maintain his present opinion, which is that this abnormality is found only in the class of skulls above named. My memorandum of the examination of a North American Fox Indian, No. 229 of the Heidelberg collection, reads, 'exostoses in the auditory canals.' Of nine skulls of Marquesan Islanders, which neither belong to the American race nor exhibit a trace of artificial deformity, I found aural exostoses in two, one of which was in an advanced stage of development. To this must be added, that in the civilized nations of Europe these exostoses are by no means as seldom as the writers on aural surgery indicate, and I believe, after thoroughly reviewing the collection described by C. O. Weber, (*Die Exostosen und Enchondrome*, Bonn, 1856,) that the *meatus auditorius externus* may be designated as a peculiarly favorite position for these growths. The appearance of these exostoses as one of the well known consequences of disease, is by no means the view of Professor S., but he regards them as peculiar to the Titicaian skulls. But I can not agree with him in thinking that the exostoses of that foreign race should be considered as any thing different from the same well known object appearing on the German skull, and recognized by aural surgeons. We are, however, indebted to the studies of Professor Seligmann for the knowledge of the certainly not uninteresting fact that these exostoses occur much more frequently in the transatlantic skulls than in those of the population of our own continent. Thus, in the examination of the skulls of foreign races, I have found the three before named cases of aural exostoses, while in the Caucasian skulls, which I have examined in a much larger number, I have not as yet met with an aural exostosis."

As to the treatment of these morbid growths we can say but little. Constitutional treatment is of no avail, and thus far local means have not accomplished much. Considering them as consequences of long existing inflammation, our therapeutic resources are mainly prophylactic. If the primary disease of the ear be attended to the growths will not occur.

The Hypo-sulphite of Soda in Scarlet Fever. Will it prevent the Disease? By N. L. NORTH, M.D., Brooklyn, N.Y.

There is a great popular dread of scarlet fever in all civilized communities—more than of any other of the so-called ordinary exanthemata. Vaccination has wonderfully mitigated the fear of small-pox, and measles is generally looked upon as a complaint of little moment. But scarlatina, having cut down the favorite flower of so many families, and left its incurable sequelæ to mar the physical powers or appearance of so many other loved ones, has come to be looked upon as a lion in the path of life by fond parents the world over. Whatever, then, may be found in the way of treatment to lessen its virulence or prevent its occurrence, will be hailed by the public, as well as by the profession, as of vast importance.

Belladonna has been claimed as a prophylactic, and very likely does exert some influence in that direction; but it is so uncertain in its effects as to have almost entirely fallen into disrepute and disuse. Domestic remedies to "prepare the system for scarlet fever," or to prevent it, are as numerous almost as the cases themselves. Some seven or eight years ago I was attending a family, when one of the children was taken sick with this disease, and, as usual, the child was medicated before the "doctor" had been sent for, and in this case "cream of tartar and sulphur" was the cure-all, and the patient had had its dose, and I was, upon my arrival, called upon for permission to have it given the other (healthy) children as a preventive. I assented, and to my astonishment, and to the great gratification and pride of the "friend of the family," who had suggested it, none of the other of the numerous children of the family were attacked by the disease. As, however, that was no uncommon occurrence, and knowing that scarlet fever does often attack one or more members of a family and not all, I thought very little of the circumstance until in the same neighborhood I saw and heard of the same thing being repeated several times with the same result, when I thought it worth while for me to try it. Accordingly I began giving the "cream of tartar and sulphur" also, to "prevent scarlet fever;" and, though it often failed in its work of prevention,

I could but think that it sometimes had prophylactic power; and believing it to be the sulphur, I concluded to combine that drug, in its precipitated form, with the extract of belladonna, and give it in all cases where children coming under my care had been exposed to the scarlatinal poison, and I believe often with the effect of preventing the disease.

After the promulgation of Dr. Pallis' theory of the use and effect of the sulphites and hyposulphites in the zymotic diseases, and after I had seen something of its use in typhoid fever, I concluded to give this remedy a trial in scarlatina, and have since given it very frequently as a remedy of much power, as I believe, in controlling the symptoms of the developed disease, by eliminating or destroying the poison, and also as a prophylactic.

On the 12th of February, 1865, I was called to attend a little child of Mr. T., of this city. The child was about one and a half years of age, and suffering with a severe attack of scarlatina-anginosa. I gave five grains of hyposulphite of soda, dissolved in syrup and water, every four hours, and ordered that the well child, who was about three years of age, should have the same dose three times a day. The patient improved rapidly and with ordinary attention soon recovered, and the other child showing no symptoms of the disease, the medicine was discontinued after five days.

In the early part of June, 1865, I was called to attend Miss S., a stout girl, of fifteen years of age, who had been exposed to and had taken scarlatina, which was, when first seen by me, fully developed. I used the same remedy in ten-grain doses every three hours, and gave five grains three times a day to a little girl of four years, who had been with her most of the time since she had been complaining, and who continued to stay with and around her during her whole sickness. The patient recovered rapidly, seeming to be favorably affected by the hyposulphite, and the little girl, with whom the medicine was continued a week, had no symptoms whatever of the complaint.

Again, by reference to my notes, I find an interesting case, commencing April 4, 1865. Mr. F. has two interesting girls, one eight and the other two years of age. The eldest was taken sick with scarlet fever, and I commenced giving, in con-

nection with other remedies, the hyposulphite of soda, in five to eight grain doses every three or four hours, and three grains three times a day to the little one. After the first day's treatment I myself was taken ill, and obliged to ask a neighboring physician to take charge of my patients, which he very kindly did, including the scarlet fever patient. After three days I again got about, and was advised by my friend, who had attended my business, to be sure to see this scarlet fever patient early, as he thought it very probable I should lose her. I did, and, of course, as I had not urged it, the hyposulphite treatment was not followed either for the child with the fever or the one exposed to it. I immediately returned to the plan of treatment I commenced upon, and in twenty-four hours thereafter my patient was much improved. I also gave the medicine to the little child, but not to my satisfaction, as it produced a cathartic effect, and I was obliged to discontinue it. About seven days from the time I was first called, the older child was fairly convalescent, but the younger began to complain, and show symptoms of the approaching malady, so that I now gave to her the medicine in smaller but frequently repeated doses, and after some three days of listlessness, with poor appetite and slight soreness of the throat, she commenced improving, and had no further symptom of the disease. About the ninth day from the attack of the first child, one of the attendants, a miss of eighteen, who had never had the fever, began to complain of headache, sore throat, &c., and was much frightened. To her I gave ten grains of hyposulphite of soda every two hours, and, after about sixteen hours, catharsis commenced with relief of the symptoms. She continued the medicine, ten grains three times a day, for four or five days, and had no further symptoms of scarlatina, except that the throat was not entirely well for four or five days.

Mr. S. has a family of five children, all quite young, none of them ever having had scarlet fever. Was called, June 27, 1865, to see the youngest, a child of two years of age, who was covered with the scarlatinal eruption, had a very sore throat, and who otherwise presented unmistakable symptoms of scarlet fever. I used the hyposulphite, with, however, not very marked good effect, so that I had to fall back on old

remedies. The child finally, after a very severe and protracted sickness, recovered. The peculiarly interesting part of this memorandum is that the other four children were given the hyposulphite, according to their several ages, and not one of them took the disease.

One other note and I will close. A Mr. B., of Wilson street, this city, has brought up a large family, and all have had scarlet fever, and suffered terribly, except two of the younger ones. I was called in haste, on November 24th, 1865, to see the youngest of these two, and found a well marked and well developed case of scarlatina. I immediately resorted to the hyposulphite of soda for both the sick one and the well one, and had the satisfaction of seeing the sick one recover rapidly from a severe form of the disease, with no other remedy than the one mentioned and some chlorine water as a gargle for the throat. The other child, although in the room with the sick one most of the time, presented no symptoms whatever of the complaint.

I am not so sanguine as to suppose that we have in the hyposulphite of soda an unfailing remedy for this dreaded malady, or even a positive prophylactic; yet I have a strong belief that it may prove beneficial both in the treatment and prevention of scarlet fever. I have hastily recorded these brief notes of cases, with the hope that they may have the effect of inducing others to try the remedy and report upon its effect.

A Case of Version by External Manipulation. By J. B. REYNOLDS, M.D., Physician to Demilt Dispensary; Visiting Physician to the Colored Home.

Mrs. C., aged 35, fell in labor with her fourth child, having had one miscarriage. From her former physician, as well as from herself, I obtained the following account of her three previous labors.

First, presented breech, child lost; the second, breech, child born nearly asphyxiated, but was resuscitated and is now living; and the third, child presented transverse, with its back anterior and the head in the left side. The child was turned in the

usual way, by introducing the hand within the uterus, seizing the lower extremities, and delivering, but the respiration could not be established.

I was called at noon of November 9th, 1865, and having been forewarned that she had never had a natural labor, I went prepared for trouble. Saw her at a quarter past one P.M., and found her in labor for five or six hours, but the pains were very feeble, and at long intervals. Examining per vaginam, I found the os largely dilated, and the bag of water pouting, but could feel no presenting part. Upon introducing my hand into the vagina and pushing up the membranes in the interval of a pain, I could feel the ribs, but, fearing lest I should rupture the membranes, I withdrew my hand to examine the abdomen externally. At this time the patient informed me that I need not fear to tell her that it was a cross-birth, for she was fully aware of it, as it was exactly like the last, where the doctor turned. The motion of the child, she complained, was very severe upon the right side, high up, and opposite to it she often felt and could see a large round mass, or ball, as she called it, roll up and down, causing the surface of the abdomen to stand out in bold relief. Upon palpation, I found a hard round mass in the left hypochondrium, a soft mass upon the right side, and a hollow between. The foetal heart was heard over the whole front of the abdomen, with summum of intensity a little to the left of and below the umbilicus. Fearing the rupture of the membranes, I concluded immediately to try to turn by external manipulation, especially as the pains were not strong, and about ten minutes apart. Being upon the right side of the patient, and having the knees flexed and abdominal wall perfectly relaxed, I pressed down the head with the right hand, at the same time pressing up the breech with my left. By steady pressure and moulding, being obliged to desist but once by a pain, in less than five minutes, and with surprising ease, the head was lodged within the crest of the left ilium, in the pubic region, while the breech occupied the left epigastric region. The uterine mass, at this time, formed a concavo-convex shaped tumor, with concavity looking to the left. The head, deep within the crests of the iliaë, could be grasped and moved by the hands externally. She now com-

plained considerably from the severity of the foetal movements in the epigastrium. The foetal heart was now most distinct a little to the right of the median line. Examining again with the hand in the vagina, by pressing up the membranes I felt the foetal head. The membranes were quite tough, and required some trouble in rupturing them. The os, before well dilated, contracted down to the size of a half dollar after the waters came away, having been twisted forward closely to the pubis, and the outlet for the waters seeming to have been carried up within the edges of the os as a second bag began to pout, but by pressure upon it the liquor amnii flowed away from within the os. The head was held in place, though there seemed no tendency to slip, for over three hours, before it fully engaged within the brim. About five P.M., I thought I felt a fontanelle and suture, but could not make out the position. Two hours later, the pains still being feeble, but the head slowly descending and the foetal heart beating regularly, I became very doubtful of the presenting part being the vertex, and at eight o'clock the chin separated and the face soon cleared the vulvæ, the chin looking up and to the left. I had converted a transverse into a face presentation. The mother and child are doing well.

Congenital Hypertrophy of the Tongue. Amputation. By
ALFRED BOLTER, M.D., Ovid, N. Y.

The subject of this malformation is a daughter of G. K., a resident of Seneca county, of this state. She is now a little over three years of age—is of large physical development, and of healthy parentage. With the exception of a severe attack of diphtheria in the fall of 1864, the child has always been healthy.

The unusual size of its tongue was noticed at its birth, and the mother says, continued to grow with its growth. It materially interfered with the process of suction, but did not wholly prevent it.

My attention was not called to the case until after the period of dentition. The tongue was then protruding from the

mouth to the extent of something more than an inch. Its appearance was tumefied, red and glossy, as if inflamed. But I soon discovered that this was not the case. There was no unusual heat, or tenderness, or febrile excitement. It was obviously a case of preternatural growth, or abnormal enlargement, and not one of *disease*, any more than an extra finger or toe would be. I advised nothing to be done except the removal, by the knife, of so much of the organ as prevented the teeth and lips from coming together. After explaining, as fully as possible, to the parents, the nature of the operation—its dangers and probable and possible results, they determined, after long deliberation, that it should be done.

Accordingly, on the 12th of December last I proceeded to the work, assisted by Doctors Post, Morris and Woodward. I should here remark that the child, at this time, was in perfect general health. But its tongue had become a much more unsightly and disgusting deformity. It was constantly dribbling with saliva, and parts of the exposed surface were blackened, dried and shriveled. Fissures traversed those parts, from which flowed considerable quantities of bloody serum. This was, no doubt, very much aggravated by the child frequently picking the surface of the tongue with its fingers. The countenance of the child was, of course, filthy and revolting, despite every parental effort at cleanliness. From the size of the tongue, the orifice of the mouth appeared nearly circular, and to be entirely filled when the features were in repose. The under lip was everted upon the chin, and the lower incisor and canine teeth, covered with tartar, were projected obliquely forward. The tongue was of firm and cartilaginous consistence, but with no unusual sensitiveness to the touch.

When every thing was made ready for the operation, the patient was put under the influence of chloroform and sulphuric ether, in the proportion of one part of the former to two of the latter. She readily became insensible. Her limbs and body were then firmly wound with strong toweling, so that all motion might be easily prevented; for in that case I was apprehensive that it might be impossible, or very difficult, to repeat the anæsthetic on account of hemorrhage. The child was then held in a sitting posture, in the lap of an assistant, and, taking

a chair directly in front, I first passed a strong ligature through the body of the tongue, for the purpose of enabling me to hold it with more facility. Then drawing the organ forward, I thrust a straight, sharp-pointed bistoury underneath, pushing it obliquely backwards and upwards, and bringing out the point near the median line, and then cutting obliquely outwards towards the canine teeth, thus making the left flap. After securing the raninal artery, the only one that required ligature, I then passed through this left flap, laterally, a double suture, for the twofold purpose of joining it to its fellow, soon to be made on the other side, and also to give me control of the organ after the part to be removed was entirely separated and the tongue retracted within the mouth.

The instrument was then again passed through to form a corresponding flap on the right side, leaving, however, a narrow central septum uncut until the bleeding vessels, two in number, were tied. This part was then divided, and the piece removed was in the shape of an inverted letter Δ .

The tongue, now forked in shape, retracted within the mouth. The next step was to draw it forward by means of the suture already passed through the left flap, and then to pass the same suture through the right flap from its inner face to the side, then approximating the cut surfaces of both flaps firmly together, and securing them in that way, by dividing the suture, and tying one part on the dorsum and the other underneath the tongue. The extremities of the flaps were then brought together by a single suture passed from side to side and tied upon the apex. This completed the operation, and a pointed, well formed tongue was made, with no part of cut surface exposed. The time consumed in the whole of this work was, about twenty-five minutes.

The piece removed was one inch and five-eighths in length, one inch in vertical thickness, and five inches and five-eighths in circumference. This was, relatively, an enormous growth.

All the cases recorded, that have fallen under my observation, have been those of adults, and while they have been described as of much larger proportions, it will doubtless be conceded that the case I have detailed exceeds them all, when the age and development of the parties are taken into the account.

The hemorrhage attendant upon this operation, although considerable, was quite easily controlled. The oozing of blood was very little after the sutures were adjusted.

The inflammation, for several days, was severe, causing the tongue to swell so largely as quite to prevent deglutition even of the blandest fluids, and rendering the child, most of the time, restless from pain. This acute state passed pleasantly away about the fourth day, when the ligatures from the arteries came off spontaneously. The treatment consisted of cold applications and washings, mainly, with a very limited use of antimonials and opiates.

The sutures were not removed until the tenth day, when the union was nearly complete.

The recovery of the child has been rapid, and the indications now are of a perfect success. The lips can already be closed, and the teeth nearly so. There is every prospect that, in a few weeks more, both will come together in a perfectly natural way, and this great deformity will never again offend the sight of the patient or her friends, or subject her to the numerous disabilities which its existence occasioned.

Surgeons have generally been deterred from amputating any considerable portion of the tongue on account of its great vascularity, and the danger of an uncontrollable hemorrhage. The success of this case, and of others that have been reported, prove that this peril is not so great as it has been supposed to be.

Cases of this kind are not of frequent occurrence—at least, few have been reported. Dr. W. G. Delaney, U. S. Navy, in a case reported by him in the *American Journal of Medical Sciences*, No. 32, October, 1848, says that his case, and two others, recorded by Dr. Thomas Harris, Phila., in the same journal, November, 1830, and May, 1837, were the only ones of the kind, to his knowledge, in the United States.

Since that time few, if any, cases have been put on record. But be this as it may, the case, in any view that may be taken of it, will, doubtless, be regarded as of sufficient interest and importance to merit a place in the annals of surgery.

PROCEEDINGS OF SOCIETIES.

NEW YORK PATHOLOGICAL SOCIETY.

Stated Meeting, October 25, 1865.

Dr. GURDON BUCK, President, in the Chair.

DEATH OF DR. D. S. CONANT.

Dr. PEASLEE expressed a desire to make some amendments to the minutes of the previous meeting, so far as they referred to the case of Dr. Conant. He had not been aware that a post mortem had been made till the day succeeding it, and, had he supposed any thing had been found which justified the bringing the brain of the deceased before this Society, he should have taken pains to have been present at the last meeting, and at that time to have given his account of the case, as he was the only physician who had been in regular attendance upon it from its beginning to its termination. The statements given in the minutes of the last meeting, he said, were very imperfect, and, in some instances, so far from conveying a correct impression, that he should be very sorry to see them published without the necessary corrections.

He then proceeded as follows:

I first saw Dr. Conant on Saturday, September 30th, one week and a day before he died. He called on me at my office, I having just arrived from the country, saying that he was pretty well, except that he had a headache and a very slight boil on the nose. This was situated on the right side, at the junction of the lateral cartilage with the os nasi. It was less than a quarter of an inch in diameter, and appeared in itself to be of no importance. I, however, proposed to him, inasmuch as it gave rise to pain in the head, to divide it freely, and did so. About half an ounce of blood escaped. This was done with a scalpel, very thoroughly, and *down to the bone*. I do not know of any other proper way to treat such affections, when very painful, than by dividing them freely to the bone. I did not see him again until twenty-four hours after, when we were leaving church together on Sunday afternoon, when I observed that the elevation and redness had entirely disappeared, and that the surface was of natural appearance. I, however, inquired, "How are you, now?" "I feel very miserably," he replied; "my head aches, and I am very languid." On looking at him more carefully, I saw that his countenance seemed bloated, that

his face presented a bronzed tinge, and that his expression was bad every way. I advised him to go home, bathe his feet in mustard-water and get to bed, and lie there until he should feel better, telling him that I thought this was the only course to pursue in order to prevent a serious illness. He said he would do so. I was subsequently kept very busy until nine o'clock in the evening, when, still feeling very anxious about him, I went around to see him. I then found a very slight flush above the incision I had made twenty-eight hours before. The incision, I may say, was about a half an inch in length. There was no tenderness or elevation of the parts, and I simply suggested a poultice, and advised him to take some triplex pills at 10 P.M.

I engaged to see him the next morning at nine, but was sent for before that time. I found that the medicine had acted thoroughly upon the bowels three times; but he then said, "I have such an agonizing pain in the left iliac region, that if I had not had such free evacuations of the bowels, I should feel quite sure that intussusception exists." I examined the parts thoroughly, and assured him that the pain was neuralgic in character. He had already made use of an enema of twenty drops of Magendies solution of morphine, and remarked that he had been in the habit of using it somewhat for the purpose of arresting discharges consequent upon an irritation of the bowels, from which he had suffered since he volunteered his services at Antietam. He was now relieved somewhat, and I advised him to use no more of it, if he could get along without it. I saw him again soon after noon, and found that the pain had shifted to the left side, at about the course of the eighth intercostal nerve. A careful examination of the chest was made, but there were no indications of pleurisy. Soon after the pain shifted to the top of the left shoulder; then it attacked the right shoulder, and finally disappeared. I at this time made a second incision, down to the bone, through the newly inflamed part. I should say, in regard to the case not being regarded as a serious one until Thursday, that I regarded it as such from Sunday evening, and it was on Monday evening that I expressed the idea that Dr. Conant was doomed; and I then, as the inflammation had again commenced, anticipated one of three results—either, first, that it would attack the eye-ball, destroy the sight, and then become arrested; or, second, that it would pass on and attack the brain, and prove fatal; or, thirdly, that in case neither of these results occurred, the septicæmic condition in which I believed him to be, would probably prove fatal at the end of ten days or a fortnight.

After dividing the tissues down to the bone the second time the inflammation again diminished, and I had a faint hope that its progress

was arrested; but I was disappointed. Twenty-four hours after, the inflammation showed itself higher up, attacking the tissues over the lachrymal sac, which presented an extreme hardness, greater, I think, than any swelling I had ever felt, even in case of inflammation of the fibrous tissues; here, however, very little fibrous tissue was involved in the process. This part was also divided freely, and the angular vein was purposely cut into, discharging about two ounces of blood. The inflammation was, however, again arrested only for a day, and on Thursday, it lighting up again, I insisted upon a consultation, and Dr. Parker saw the case, who, after examining it thoroughly, remarked that there was nothing additional to be done.

In regard to the formation of pus, I should say that none at all was formed within the first forty-eight hours; and when formed it was perfectly healthy in appearance. A little would accumulate, and was twice daily pressed out of the incision. On one occasion a little pus had collected at the lower portion of the cut, and this formed a little sinus capable of admitting a small probe, which extended down to the tip of the nose. This was opened, and had entirely healed up two or three days before death. The site of the original inflammation, also, was then entirely healthy again.

The pulse, after Monday, when it was 108, and Tuesday, 106, came down to 80 or 85, and remained so most of the time for three days, so that when Dr. Parker first saw him, on Thursday, his condition was comparatively good. There was also not much swelling around the orbit until Friday, when, for the first time, it became soft and quaggy, much as we see in a low case of erysipelas. Exophthalmus had occurred Friday evening, and the sight was lost by Saturday forenoon. The other essential facts of the case were detailed at the last meeting of this Society.

In regard to the nature of the local affection there has been a great diversity of opinion. We may often very accurately appreciate the pathology of a case, and even predict the progress and the termination of the malady, though it may be difficult to apply to it a name. A name is nothing but a term applied to a class of cases, and all the cases in the class can never be alike. Still, there must be a typical case, though it exist only in the mind—a *central idea*—around which all must be grouped. A name is, then, a mere band for a bundle. We must have, from the nature of things, some cases in a class which are not typical; and, therefore, cases which it is difficult to assign to any class, since they touch on the confines of two or more cognate classes. When the sister of Dr. Conant came to Dr. Parker with the

word that her brother had the so-called malignant pustule, that statement was not based upon any opinion which I had formed of the case. It seemed at first to be a mere furuncular inflammation, and never, to my mind, assumed the character of malignant pustule. The inflammation entirely disappeared from its original site, and the part got well in three or four days, while the malignant pustule always extends around from the original starting point, generally producing mortification and sloughing.

Then, again, if we call it erysipelas, we find, though like it, it traveled from point to point, it was nevertheless always confined within narrow limits, and followed every where the fibrous tissue, and the course of the veins and lymphatics. It was more like *phlebitis*, in most respects, and if I were obliged to call it by one name, I would call it *phlebitis*. When I was called upon for a certificate, finding that I could not express my ideas of its pathology without a circumlocution, and it being evident to me that inflammation of the membranes of the brain was the immediate cause of death, I gave a certificate to that effect. I said inflammation of the brain, though that by no means expresses my whole idea, for it is not a mere case of cerebral meningitis. Diffuse cellular inflammation is another term that might be applied, but it commenced in the fibrous structures, and only secondarily involved the areolar tissue. I should say that the local affection resembled *phlebitis* nearer than any thing else. But he had all the indications of blood poisoning from the onset, and I think if that condition of the blood had not existed, and which had been produced by his incessant labor and exposure in August, this inflammation, call it *phlebitis*, or whatever you may, would not have progressed so far but that a healthy exudation would have arrested it. The inflammation passing through the sphenoidal fissure, which gives passage to the ophthalmic vein, and not through the optic foramen, is another fact in favor of the idea that the local affection was *phlebitis*. In regard to the statement that pus existed at the base of the brain, I must reserve to myself some doubt. All know how difficult it is to distinguish between an exudation and pus in cases of a low grade of inflammation, and in this case no symptoms of meningeal inflammation occurred till fifteen hours before death.

TUMOR FROM SHEATH OF SCIATIC NERVE—DR. L. A. SAYRE.

Dr. SAYRE presented a bony looking mass, which was rounded and about the size of an English walnut, which he had removed from a

patient forty-five years of age, with the following history: He consulted Dr. Sayre during the month of July last, complaining of a severe pain in the left hip joint, and stated that he had suffered from the symptom for a period of three or four years. Hypodermic injections of morphine had been resorted to, with other local remedies, and no relief had been obtained. On careful examination no disease of the joint was made out, but on flexing the thigh strongly upon the pelvis, a hardened mass, which was movable, was felt behind the greater trochanter, immediately over the most superficial portion of the great sciatic nerve. On cutting down upon it for the purpose of removal, the mass was found connected simply with the sheath of the nerve. There was no assignable cause for its appearance, no gouty tendencies in the patient, and the distressing symptom of pain entirely ceased after the offending substance was removed. No microscopic examination of the tumor had been made.

Dr. PEASLEE remarked that if the tumor were found to be made up of nothing but fibrous tissue, with calcareous deposit, it would not be so unusual; but if composed of true bone, possessing lacunæ and canaliculi, it would certainly be unique in being developed from a nerve sheath.

On motion, the specimen was referred to a committee, consisting of Drs. Sands and Draper, for microscopic examination.

Stated Meeting, November 8th, 1865.

Dr. H. B. SANDS, Vice-President, in the Chair.

TUMOR FROM THE NARES—DR. KRACKOWIZER.

Dr. KRACKOWIZER presented a portion of a tumor, which he had tried that morning to remove from a patient twenty-five years old, whom he had seen, for the first time, about six years before, in the Jews' Hospital. At the time he was suffering from ozæna, the discharge coming from both nostrils. In consequence of this the upper lip was eroded, and was covered with a crust of inspissated matter. In the course of a few weeks cauterizations and mild astringent applications succeeded in arresting the disease of the nose and remedying the trouble on the lip. There were no hereditary diseases in his family. After he left the hospital he embarked in various avocations until the close of the last year, when he became conductor on one of the Philadelphia railroad lines. Late in December of last year, in pretty cold weather, he stood for some time in the sleeping car, when he found it quite warm

there, and, in order to cool himself, went out upon the platform and unbuttoned his coat. Two or three days subsequently he was taken with a violent hemorrhage from the left nostril. For the first time he then noticed a swelling in this nostril, which swelling gradually increased during a period of two or three months afterwards, so that no air could enter the passage. The tumefaction could readily be seen by him with the aid of a looking-glass, and could be easily felt with the tip of the little finger, and was covered with a mucopurulent discharge.

In July, about the sixth month after the first hemorrhage, he was attacked, and was, in consequence, left very weak. From that time, he says, the swelling gradually became arrested, so that he could not reach it at all with the finger. About that time a little tumefaction appeared at the inner angle of the eye, pushing the eyeball forward and downward. This swelling increased, so that, at the time Dr. Krackowizer saw the patient, the level of the cornea was protruding beyond the orbital margins, and the eyeball was barely covered by the lids. The eyeball was not altered in appearance, and could be moved in every direction at will. He had abandoned his position as railroad conductor, and had settled in Virginia for several months. Finally, becoming alarmed at his condition, he came to this city. Dr. Krackowizer saw him about a week ago.

He seemed to be in perfect health, and stated that he had never suffered any from pain. The condition of the eyeball, as just described, was noticed, and in addition a marked swelling at the inner angle of the eye, which, by its pressure, interfered with the absorption of the lachrymal secretion through the canaliculi, and the eye was, consequently, watery. The whole left side of the face was a little protuberant, and, on feeling into the mouth, the anterior portion of the superior maxillary bone was discovered to be unusually full, soft, and elastic; and on one part, in the region of the canine fossæ, it seemed that a layer of thin bone was spread on the surface of the swelling. The interstices between the coronoid processes and the outer surfaces of the superior maxillæ were equal. The temporal fossæ were the same on both sides. On looking into the nose, no swelling could be seen. The soft palate was normal. Dr. Simrock examined the posterior apertures of the nose with a rhinoscope, and found the equina filled with a mass, as is usually seen in cases of new formations in the nasal cavities. The submaxillary and cervical glands were not infiltrated. As the patient had had two or three pretty smart attacks of hemorrhage from the nostrils, it was not deemed advisable to probe

the parts much. The opinion was that it was non-malignant in character, and had originated in the walls of the antrum.

It was decided to remove the mass. An incision was made, commencing at the root of the nose, along the median line, leaving the septum on the right side and splitting the upper lip. The flap was then dissected from the neighboring parts, but the hemorrhage being quite considerable, there was a good deal of necessary delay in the operation in consequence. After the flap had been dissected from the anterior surface of the superior maxillary bone, from the nasal process of this bone, from the nasal bone of the left side, and from that part of the tumor which appeared in the orbit, always taking care not to wound the eyeball or its muscles, it was found that in order to get a good access to the mass, another incision from the angle of the mouth to the malar bone had to be made. This flap was then dissected off and turned above. Langenbeck's saw was then passed through a hole in the outer aspect of the maxillary bone, and a transverse incision made along the floor of the antrum to the pyriform aperture of the bone. Then, with a strong bone scissors the bridge of the nose, between the inferior orbital margin and aperture of the nose, was cut in two, then the connection between both nasal bones was divided, and this bony bridge was luxated from the suture which connected it with the os frontis. The tumor was then exposed. Another incision was then made in the floor of the orbit, from between the outer and middle third of the orbital margin, in an oblique direction towards the inferior orbital fissure. By that means the mass became movable, and was twisted from its surroundings. After this was done, it was found that the whole of the antrum, as well as the nasal cavities, were filled with this mass. It at once became evident that it would be impossible to remove all of the disease, and so as much was taken away as could be conveniently done by means of the finger and scraper. In that way it happened, not, however, as the result of any force used, that that portion of the diseased mass situated anteriorly to the cells of the sphenoid, and taking the place of the bone in that situation, was scraped away, laying bare the dura mater. The patient, notwithstanding the loss of a good deal of blood, had a good pulse at the end of the operation. The flaps were brought together by means of a twisted suture.

Under the microscope the elements of the tumor consisted, in part, of elongated cells, with a single nucleus, arranged in pretty regular parallel courses, and also elements the size of pus corpuscles, with one nucleus, all of which proved that the growth was entitled to be classed

under the head of soft sarcomas. Though tumors, these were not to be considered cancerous. They nevertheless partake of the malignant character of such growths, as far as their tendency to recur was concerned, more especially when the slightest trace of disease was left after an operation.

In regard to the prognosis in this case, it was evident that death must ensue from meningitis, the result of the accidental injury of the dura mater.

ANEURISM OF THE THORACIC AORTA—DR. C. K. BRIDDON.

D. C., age 38 years, came under my observation about six months ago; he was complaining of pain shooting through the chest, from a point one inch to the left of the sternum, and between the fourth and fifth costal cartilages, to another situated below the angle of the left scapula; he had slight cough, with little or no expectoration, experienced some uneasiness in the stomach after eating, had a coated tongue, irregular bowels, and the usual common symptoms of derangement of the chylo-poietic viscerae. To these matters he appeared to invite little attention; his chief complaint was of the chest pain, and, on close questioning, I elicited its character; it was situated between the two points above mentioned, did not radiate, and was not, properly speaking, lancinating, but was steady, persistent, and sufficiently severe to interfere with or prevent sleep. It was not influenced by deep inspiration, position or motions of the trunk or extremities; there was no tenderness in the intercostal spaces, either in the immediate neighborhood of, or at a distance from, the seat of pain. Physical exploration gave no insight into the nature of the case; anteriorly and posteriorly percussion was equally clear on both sides. Auscultation revealed no rales in the pulmonary or bruits in the cardiac region of the chest.

The patient remained under my care a little while, and then moved into the country. About two months ago I visited him in consultation with the physician who was attending him at Bayonne, in the State of New Jersey. He had had a severe attack of gastralgia, but the old chest pain was still present, and his chief complaint. I again made a careful physical examination, and, beyond some difference in the percussion note beneath the clavicles, detected nothing.

I next saw the patient in this city on the 26th of October, when he informed me that he had consulted Profs. Parker and Clark, and had been a short time under the care of the former. When I visited him

he was suffering severely from his old enemy; during my conversation with him I became impressed with the idea that he had some grave trouble in one of the mediastinal spaces, probably aneurismal in character, but he appeared to be so much exhausted by pain that I deferred an examination for the following day.

At half-past eleven o'clock on the night of the 26th, I was again summoned, and on reaching his house found that he had died about fifteen minutes before my arrival. His mode of death was sudden; after an hour's aggravated pain he cried out that he was dying, and fell back dead.

Autopsy, thirty-four hours after death. The examination was confined to the contents of the chest. On elevating the sternum, I found the left lung irregularly displaced upwards, backwards and inwards, by a coagulum of blood estimated at six or eight pounds, and a quantity of serum; there were a few old adhesions, but they were neither numerous nor extensive in character. There were a few solid tubercular masses in either apex, and another deposit of the same character in the anterior border of the lower left lobe.

On removing the viscerae from the chest, an aneurismal dilatation was found occupying the anterior wall of the thoracic aorta; the left bronchus was closely applied and intimately adherent to the front or external surface of the sac, and to the left of the bronchus was a ragged opening which admitted the forefinger into its interior. On slitting up the posterior wall of the vessel which was the seat of advanced atheromatous deposit, the orifice of the sac was disclosed; its upper border was on a level with the junction of the arch with the descending aorta. It measured two inches and three quarters in its vertical and one inch and a half in its transverse diameter; the cavity into which it led would contain an average-sized lemon. It contained neither clot nor laminated fibrine, and projecting into its cavity could be seen and felt some of the posterior extremities of the cartilages of the left bronchus, covered by the lining membrane of the vessel.

In conclusion, he referred to a case of abdominal aneurism, which strikingly resembled the one just related, in having a persistent pain, as the only symptom which showed itself for a long time.

Dr. O'SULLIVAN remarked that he had a case, then under treatment, which resembled the one related by Dr. Briddon, except that there was no pain present. He could detect dullness on both sides of the sternum, high up, and further than this there were no other physical signs appreciable. Adjourned.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Circular No. 6. War Department, Surgeon-General's Office, Washington, November 1, 1865. Reports on the Extent and Nature of the Materials Available for the Preparation of a Medical and Surgical History of the Rebellion. Printed for the Surgeon-General's Office, by J. B. Lippincott & Co. Philadelphia, 1865. 4to. pp. 166.

A good deal of concern has for some time been felt by the medical profession of the country—a large part having a direct interest in the matter—about the progress that had been made towards gathering, preserving, and digesting for publication the mass of facts and observations in military medicine and surgery which it was known had been accumulated in the Surgeon-General's office. A perusal of these opportune "Reports" will put to rest any anxiety or doubt that may have been felt as to the means taken to secure the early and full fruition of the enormous experience gained by four years of war, carried on by armies of unparalleled numbers, and its utmost utilization for the general good. It will also give assurance of the fitness of the officers to whom this important and difficult task has been entrusted—the Medico-Chirurgical Historiographers of the War; and check any impatience at tardiness of publication, by revealing the magnitude of the subject, showing the measures adopted to secure accuracy and thoroughness, and bringing the conviction of the absolute necessity of the patient investigation of many points, and that time will only add increase to the truth of these mighty stores. The "Reports" are a triumphant vindication of the Medical Department of the Army, and a noble monument to the courage, zeal, ability and acquirements of its officers.

The Report on Surgery is by Bvt. Lieut-Col. GEO. A. ORIS, Surgeon U. S. Vols., and the Report on Medicine by Bvt. Major J. J. WOODWARD, Assistant-Surgeon U. S. Army. A full analysis of them is not practicable within our limits, and our readers must be content with an idea, necessarily imperfect, of their scope and value.

The materials in the Surgeon-General's office from which the *Surgical History of the Rebellion* is to be compiled, and whose extent is stated as "simply enormous,"

"Consist of the reports of the medical officers engaged in it, and of illustrations of these reports in the shape of pathological specimens, drawings and models. The documentary data are of three kinds: first, the numerical returns, in which the number alone of the different

forms of wounds, accidents, injuries, and surgical diseases is given; secondly, what may be called the nominal returns, in which are furnished the name and military description of each patient, and the particulars of the case, with more or less of detail; and, thirdly, the miscellaneous reports," (p. 1.)

We have the gratifying information that "the great body of the medical officers have made the reports required of them with commendable diligence and promptitude," and their industry and zeal are the more praiseworthy when the absorbing nature of their hospital and field duties is considered.

In the British army in the Crimea there were 12,094 wounded, and 2755 killed, or a total of 14,849. In the French army, whose total effective force was 309,268, there were 39,868 wounded, and 8250 killed, or a total of 48,118.

"In the late war, the monthly reports from a little more than half the regiments in the field give, for the year ending June 30th, 1862, an aggregate of 17,496 gun-shot wounds. The reports from rather more than three-fourths of the regiments, for the year ending June 30th, 1863, give a total of 55,974 gun-shot wounds. The battle-field lists of wounded for the years 1864-65 include over 114,000 names. But these returns are to be completed by collating with them the reports of general hospitals, where many wounded were received whose names the recorders of field hospitals or regimental medical officers failed to obtain, and by adding the names of those killed in battle," (p. 2.)

In the French Crimean army there were 459 gun-shot fractures of the femur reported, and in the British army 194, while over 5,000 cases of this injury in our armies have already been sent in to the Surgeon-General's office. The Crimean returns give 16 exsections of the head of the humerus in the British, and 38 in the French army, "but the registers of this [S. G.] office contain the detailed histories of 575 such operations."

"The surgical specimens of the Army Medical Museum number 5480; and not only in specimens of recent injuries, but in illustrations of reparative processes after injury,* of morbid processes, of the results of operations, and of surgical apparatus and appliances, this institution is richer, numerically at least, than the medico-military museums of France or Great Britain," (p. 3.)

And these great treasures have been classified and arranged so as to be available for present scientific study.

Such materials, from their nature and extent, must necessarily throw light on many mooted points, and go far towards the solution of certain surgical problems, comprising, as they do, on some subjects—

excision of the head of the femur after gun-shot injury, for example—larger data than are extant in the whole range of surgical literature.

We will now proceed to notice the records of special injuries, and the operations done for their relief.

The number of *Gun-shot Injuries of the Head*, reported to October 1, 1864, is 5046, and they have been recorded in two classes: first, the gun-shot fractures and injuries of the cranium, including the perforating and penetrating and depressed fractures, the fractures without known depression, and the contusions of the skull, resulting in lesions of the encephalon; and, secondly, the simple contusions and flesh wounds of the scalp. In the first class 1104 cases are recorded. Of 704 in which the results have been ascertained, 505 died and 199 recovered. In 107 of these terminated cases the operation of trephining was performed, with 60 deaths and 47 recoveries. In 114 cases fragments of bone or of foreign substances were removed by the elevator or forceps, without the use of the trephine; and of these 61 died and 53 recovered. The gun shot contusions and wounds of the scalp number 3942, of which 103 terminated fatally. So far as ascertained, the fatal results were due to concussion or compression of the brain, or to the formation of abscesses in the liver or lungs, in consequence of inflammation in the veins of the diploë. Compression resulted either from extravasation of blood, or inflammation of the brain or its membranes, or from suppuration. In a case of scalp wound received Nov. 27, 1863, no cerebral symptoms occurred until Dec. 13th, 1863, when the man was suddenly seized with convulsions, followed by coma. The skull being laid bare at the seat of injury, and the bone found diseased, the trephine was applied. There was matter beneath the bone, and oozing from the diploë. "It was thought expedient to make five perforations with the trephine, in order to remove the diseased bone and give free exit to the pus. Convulsions did not recur, but the comatose condition continued, and the case terminated fatally twelve hours after the operation. The autopsy revealed diffuse inflammation of the arachnoid and of the dura mater." This is another case proving the fallacy of the doctrine taught by Pott, that the inflammation is frequently limited to the outer surface of the dura mater, and supporting the views of Mr. Prescott Hewett, that where there is found inflammation on the outer surface of the dura mater, there is also found inflammation on the free surface of the arachnoid.* And

* Injuries of the Head. By Prescott Hewett, Esq., Surgeon to St. George's Hospital, in Holmes' System of Surgery, v. ii., p. 101.

he adds: "Indeed, the successful issue of a case of trephining for matter between the bone and the dura mater is, I believe, all but unknown to surgeons of our own time."

In the Army Medical Museum there are eight specimens of that rare and interesting variety of fracture of the cranium in which the external table is unbroken, while the vitreous table is fissured and sometimes depressed. In one of the cases, Surgeon Bontecou, who had examined some of the specimens already collected, inferring the probability of a depression of the inner table, though there was no apparent fracture of the outer one—the bone being only denuded of its periosteum—verified the diagnosis during life, by the application of the trephine.*

"It is believed that this accident results, in most instances, from a small projectile striking the cranium very obliquely, or possibly, in some cases, from a comparatively slight blow from a body with a large plane surface," (p. 12.)

Closely allied, clinically, to these cases, are those in which a ball produces linear fissure of the external table with displacement of the inner table. A case, with an excellent illustration, is given. Of fractures without depression, the cases in which the mastoid process is knocked off by a ball afford examples. One case of recovery from this injury is given. Several instances of undepressed fracture are reported, in which a ball gouged out a small portion of the external table. It often happens that considerable portions of the calvaria are removed by explosions of shell, without causing depression. A remarkable case is related where a musket ball produced the same effect—entering "a little outside of the left frontal protuberance, and passing backwards and upwards, removed a piece of the squamous portion of the temporal bone, with brain-substance and membranes." The man "recovered perfectly," and "the mental and sensory faculties were unimpaired."

An abstract of a case of "punctured fracture" of the os frontis, by a pistol ball, is given, and in which there was the usual absence of all alarming symptoms until immediately before the inevitable fatal result, when trephining is not early resorted to. "Instances were not uncommon of the splitting of round musket balls in striking the skull at

* Unhappily, an abscess had already formed in the brain, and the operation, though done as soon as evidence of compression existed, was too late to save the patient. There are but two other known specimens of this rare form of injury—one in the Dupuytren Museum, and the other in the Netley Collection.

an acute angle. Conoidal balls were less liable to split after this fashion, yet such instances were occasionally observed," (p. 14.)

Recovery after penetrating, perforating fractures of the cranium was exceptional. In rare instances the fatal termination was very long delayed. A curious case is narrated where the presence of a ball within the cranium was unsuspected during life. It had entered Dec. 13, 1862; there were no cerebral symptoms until Feb. 10, 1863, when delirium set in, followed by coma, and death at midnight of the 15th.

"The autopsy revealed a conoidal musket ball wedged between the sphenoid and the left orbital plate of the frontal bone, and lying in contact with the dura mater. The orbital plate was pressed inwards and fractured, and a fissure extended through the superciliary ridge. Over the ball, at the base of the anterior lobe of the left hemisphere, was an abscess containing two drachms of pus. The ball was incrustated by callus, and the opening it had made in entering through the walls of the right orbit was greatly diminished by osseous deposition," (p. 15.)

The following is a remarkable case of perforating fracture of the skull, with recovery:

"The ball entered a little to the right of the occiput, and passed out somewhat below and to the left of the vertex. The intervening bridge of bone was about three inches wide. There had been a cerebral hernia at the wound of exit, and, when the drawing was made, four months after the reception of the injury, there was still a small tumor, covered by half-formed cicatricial tissue. There was a firm depressed cicatrix at the aperture of entry. There were no evidences of impairment of the cerebral faculties. The man was employed as an orderly," (p. 15.)

Eighteen cases of *Hernia Cerebri* are mentioned occurring in gunshot fracture of the skull, complicated with meningeal and cerebral laceration.

"In four of these cases, recovery took place without operative interference with the protruding fungous mass, which, in these instances, gradually contracted, was then covered by granulations, and finally cicatrized. In those cases in which bandaging and compression were resorted to, cerebral oppression was soon manifested, and stupor and coma eventually supervened. In those in which the tumor was sliced off, as usually recommended, at the proper level of the brain, it was commonly speedily reproduced, and death from irritation ensued," (p. 17.)

In the treatment of cranial fractures, it would appear that "the general tendency was to the practice recommended by Guthrie in regard to operative procedures, rather than [to] the more expectant plan insisted upon by the majority of modern European writers on military surgery."

Though the number of fatal results after trephining was very great, there were numerous examples of success, while the data are not sufficiently complete to admit of a fair comparative analysis.

"Still it is difficult to avoid the impression that a larger measure of success has attended this operation in the late war, than the previous experience of military surgeons would have led us to anticipate. Surgeon D. W. Bliss, U. S. Vols., alone has reported eleven successes after the use of the elevator or trephine. Even in those almost hopeless cases in which compression of the brain follows a gun-shot injury of the skull at a late date, instances of recovery are reported," (p. 16.)

Of the 1329 entered cases of *Gun-shot Wounds of the Neck*, ultimate results have been ascertained in 546 only. In the terminated cases the mortality is 14 per cent.

"Several instances are recorded in which large grapeshot, on striking the hyoid bone, were deflected, and buried themselves in the supraspinous fossa of the scapula, or among the muscles of the back. These patients died from laryngitis or œdema of the glottis, and might have been saved, perhaps, by tracheotomy; but they died suddenly when surgical assistance could not be immediately procured," (p. 20.)

In 187 cases of *Gun-shot Fracture of the Vertebra*, all but seven were fatal. Six of these were fractures of the transverse or spinous apophyses.

"The seventh case is that of a soldier wounded at Chickamauga, September 20th, 1863, by a musket ball, which fractured the spinous process of the fourth lumbar vertebra, and penetrated to the vertebral canal. The ball and fragments of bone were extracted at a Nashville hospital. The patient was transferred to Louisville, thence to Jefferson Barracks, Missouri, thence to Madison, Indiana, and finally, on July 26th, 1864, to Quincy, Illinois. The last report states that he was likely to recover," (p. 21.)

An interesting case is given in which after death it appeared that the spinal cord had been completely severed at the seat of injury, and to have become disorganized above and below. The man was wounded on the 3d of June, by a conoidal musket ball, which shattered the transverse and articular processes of the eight and ninth dorsal vertebrae, penetrating the vertebral canal. There was immediate loss of all sensation and motion below the wound. He remained in a feeble state, with slow pulse, labored respiration, cool, clammy and cyanosed skin, and involuntary fecal and urinary discharges, until the 27th of June, when excessive gastric irritability came on, and all nourishment was promptly rejected by the stomach, and he died, on July 2d, thirty days after the reception of the injury.

"Five thousand one hundred and ninety-five gun-shot flesh wounds of

the back have been recorded, of which a large proportion are injuries from shell. Troops being often ordered to lie down under a shell fire, this region becomes particularly exposed," (p. 21.)

Results have been ascertained in 1272 cases of *Penetrating Gun-shot Wounds of the Chest*, of which 930 were fatal, or 73 per cent.

"In the treatment, venesection appears to have been abandoned altogether. Hemorrhage was treated by the application of cold, perfect rest, and the administration of opium. These measures seem to have proved adequate generally, and no instances are reported of the performance of paracentesis or of the enlargement of wounds for the evacuation of effused blood. Hemorrhage from the vessels of the costal parietes has been exceedingly rare, and, in the few instances recorded, was a secondary accident. Hence the management of bleeding from wounded intercostal arteries has presented theoretical rather than practical difficulties.

"It has been the common practice to remove splintered portions of fractured ribs, and to round off sharp edges that were likely to wound the pleura or lung. After this, with the exception of extracting foreign bodies whenever practicable, and performing paracentesis when empyema was developed, it has been usual to leave these cases to the natural process of cure.

"The records of the results of the so-called method of 'hermetically sealing' gun-shot penetrating wounds of the chest are sufficiently ample to warrant an unqualified condemnation of the practice. The histories of the cases in which this plan was adopted have been traced, in most instances, to their rapidly fatal conclusion," (pp. 21-2.)

There is only one exception of reported recovery.

Where the track of the ball passed near the root of the lung recovery was rare. The cases in which there was fracture of the rib at the wound of entry were very dangerous. There was ample confirmation of the received opinion, that penetrating wounds with lodgment of the ball, are more fatal than simply perforating wounds. A remarkable recovery is mentioned, where the anterior mediastinum was opened.

"A private was struck by a three-ounce grapeshot, on the morning of May 3d, 1863. The ball comminuted the sternum, at the level of the third rib, on the left side, and tore through the costal pleura. It remained in the wound and was removed by the patient. Through the wound the arch of the aorta was distinctly visible, and its pulsations could be counted. The left lung was collapsed. When sitting up there was but slight dyspnoea. Several fragments of the sternum were removed, and the wound soon granulated kindly. On July 5th, the patient was transferred to Washington, convalescent. He ultimately recovered perfectly," (p. 23.)

Only four cases are recorded of *Gun shot Wounds of the Heart*, that came under treatment. In a case where a small pistol shot entered the

left ventricle and passed out through the right auricle, the patient survived twelve hours.

Four instances of recovery from a formidable gun-shot wound, involving both the thoracic and abdominal cavities, are given. In the first, a conoidal musket ball went through the belly of the biceps of the right arm, entered the chest, traversed the base of the right lung and the diaphragm, wounded the intestines, and passed out above the anterior superior spinous process of the left ilium. When admitted to the field hospital, he had dyspnœa and bloody sputa, and there was a fecal discharge from the wound of exit. Treated by large doses of opium, at the end of three weeks convalescence was fairly established. In the course of a few months his wounds had entirely closed, and he rejoined his regiment for duty. In another—

“A round musket ball, fired from a distance of about one hundred and fifty yards, entered the eighth intercostal space of the left side, at a point nine and a half inches to the left of the extremity of the ensiform cartilage, and fractured the ninth rib. Without wounding the lung, apparently, the ball passed through the diaphragm, and entered some portion of the alimentary canal. Captain S—— walked a mile and a half to the rear, and entered a field hospital. On examining his wound, the surgeons found a protrusion of the lung of the size of a small orange, which they unavailingly attempted to reduce. The wound was enlarged, and still it was impracticable to replace the protruded lung. Fruitless efforts were again made to reduce the hernial tumor, after which a ligature was thrown around its base and tightened. A day or two subsequently the patient passed into the hands of Surgeon Tomaine, who removed the ligature from the base of the tumor. A small portion of gangrenous lung separated and left a clean granulating surface beneath. On May 7th, the ball was voided at stool.

“There was an entire absence of general constitutional symptoms; no cough, no dyspnœa, no abdominal pain; the bowels were regular and appetite good. The protruding portion of the lung was carnified, and and there was a dullness on percussion and absence of the respiratory murmur in a zone an inch and a half in width around the circumference of the base of the tumor. It was at this date half the size of an egg, and covered with florid granulations. On June 2d, Captain S—— was transferred to Washington. There was an elastic, partly reducible tumor, over which was an oval granulating surface, an inch and a half by three-quarters of an inch. The vesicular murmur was perfect throughout the lung, except in the immediate vicinity of the tumor. After a furlough of sixty days, Captain S—— was again examined. The wound had entirely healed; the respiratory sounds were normal; there was still a slight hernia of the lung. The general health of the patient was excellent,” (p. 24.)

In a third case, the liver as well as the lung would appear to have been implicated. On the 27th of August, a man was struck by a musket

ball four inches above the crest of the right ilium, and six inches from the spine, it passing upwards and inwards, and lodging. There was cough, with bloody expectoration and crepitant rhonchi in the lower lobe of the left lung; a profuse discharge of bile from the wound, and severe pain and tenderness in the hepatic region. Acute pulmonic symptoms subsided after a week, but cough, with purulent expectoration, persisted for months.

"Early in October the discharge of pus and bile from the wound began to diminish, and in November the pain ceased in the hepatic region, and was referred to the immediate vicinity of the orifice of the wound. The patient now began to walk about the ward. Throughout the treatment there was great tendency to constipation, which was obviated by enemata. In the middle of December the cough had nearly disappeared, and there was but a scanty discharge from the wound. On January 12th, 1863, the wound was entirely healed, and the patient was discharged from service," (p. 24.)

Of 2707 *Gun-shot Wounds of the Abdomen* reported from the beginning of the war to July 1st, 1864, there were 2164 flesh wounds, and 543 cases in which the peritoneal cavity was penetrated or the abdominal viscera injured.

"Among the flesh wounds, 114 fatal cases are recorded, which were, in most instances, cases of sloughing from injuries of the abdominal parietes by shells. The number of recoveries is unexpectedly large, but includes only cases in which the reports showed, beyond question, that the abdominal cavity had been involved.

"In many instances fecal fistulæ were produced. They commonly closed after a time, without operative interference, reopening at intervals, and then healing permanently.

"Recoveries after wounds of the large intestines have been much more numerous than after wounds of the ileum or jejunum. No case has been reported in which it was thought expedient to apply a suture to the intestines after gun-shot wounds. Gun-shot wounds of the liver were usually followed by extravasation into the abdominal cavity and rapidly fatal peritonitis. Of 32 cases in which the diagnosis was unquestionable, all but four terminated fatally," (pp. 24-6.)

All the cases of *Gun-shot Wounds of the Spleen*, that have been reported, were fatal.

"Gun-shot wounds of the bladder, when the projectile entered above the pubes or through the pelvic bones, have proved fatal, so far as the records have been examined. There are many examples of recovery, however, from injuries of the parts of the bladder uncovered by the peritoneum. Several examples of recovery, after protrusions of the abdominal viscera through gun-shot wounds, have been reported. In two cases in which loops of small intestine issued, they were immediately returned and retained by means of adhesive strips and bandages,

and the patients recovered with ventral hernia. The escape of omentum, through wounds, would not appear to be a very serious complication, for in many cases portions of protruding omentum have been excised, and the patients have, nevertheless, recovered promptly," (p. 27.)

The returns corroborate the observation of Stromeyer, that there is great liability to pyæmia after gun-shot injury of the pelvic bones, tedious suppuration usually ensuing. Slight hurts to the ilium, as grooving of the crest by a musket ball, usually did well; and there were many examples of perforation of the body of the bone, with ultimate recovery.

In *Gun-shot Wounds of the Upper Extremities*, progress to any extent has only been made with the class including fractures of the shaft of the humerus and either of its articular extremities.

"This comprises 2408 cases of gun shot fractures of the humerus that have been examined and recorded. Recovery followed in 1253 cases, death in 436, and the result is as yet undetermined in 719 cases. In the 1689 completed cases, amputation or excision were practiced in 996, and conservative treatment was adopted in 693, with a ratio of mortality of 21 per cent. in the former and 30 per cent. in the latter. But it is premature to make deductions," (p. 29.)

Gun-shot Wounds of the Lower Extremities.—Only those cases of gun-shot fractures of the femur, in which the histories of the cases had been carefully scrutinized, and in which the locality and extent of the injury were clearly designated on the reports, have been entered on the permanent records—numbering 1823 cases, on September 1st, 1865, or about one-third of the total number that have been reported. Of these 1823 cases, the results have been ascertained in 1223. Of the 1183 cases of gun-shot wounds of the knee joint, the results are known in 770. Though the statistics tending towards the settlement of some of the most important, and still open, questions of surgery, are yet too incomplete to enable results to be discussed understandingly, it has been "thought expedient to report them."

"The only recorded recoveries after gun-shot fracture of the femur involving the hip joint are those in which excision was practiced. In fractures of the upper third, the mortality rate is greatest for the cases treated by amputation. There were 43 of these cases, and in 19 of them the amputation was done at the hip joint. Excision gives 7 recoveries after fractures of the upper third; 2 of these were excisions of the head and a portion of the shaft of the femur, 4 were normal excisions of the continuity, and 1 was a removal of fragments and rounding off of sharp edges of bone, which was admitted among the excisions with some hesitation. Under conservative measures 93 cases of fracture of the upper third had survived the injury a year or more, and are

reported as recovered. The mortality rate of the completed cases of amputation for gun-shot wounds of the knee joint is large, and will probably be modified when the results of the numerous unfinished cases are recorded. It depends partly, however, upon the excessive mortality of intermediate amputations of knee joint injuries. With six or eight exceptions, the 50 recoveries without amputation classified with gun-shot wounds of the knee joint were examples of fractures of the patella, in which the evidence that the joint was opened was not unequivocal. Comparing in gross the 822 finished cases treated by amputation, with the 1117 treated by conservation, the mortality rate of the former has the advantage by 8 per cent.—an advantage that is maintained in the different regions, except in the upper third. It must be remembered that the amputations include most of the bad cases, and those in which preservation of the limb was attempted and abandoned," (p. 32.)

In Stromeier's classification of the action of bullets on bone, the fifth division is that in which the ball pierces the bone and forms a canal without causing further splintering. Examples are common in the upper portion of the tibia, but very rare in the upper extremity of the femur. Specimen 565 A. M. M. shows the upper extremity of a left femur perforated by a conoidal ball. The wound was received June 26th, 1862, and death occurred from exhaustion, August 19, 1862. The near proximity of the ball had not induced any disease of the hip joint.

The degree of difference in the effect upon bones of the impact of round musket balls, and of cylindro-conical ones, has, it is believed, been exaggerated. Attention is called to one curious effect, not generally noticed, occasionally caused by the heavy conoidal ball striking the femur—the bone is fissured and comminuted, though less than is common, at the point on which the ball impinges, while at two or three inches above or below this point, according as the point of impact is below or above the middle of the shaft, a nearly transverse fracture of the shaft is produced. There are specimens in which the ball has struck the condyles anteriorly, and the shaft is snapped across two inches above. In several of these specimens the transverse fracture is not connected by fissures with the comminuted fracture produced by the ball. It would seem that these injuries were produced by balls fired at short range.

The series in the Army Medical Museum illustrating the reparative efforts of nature after gun shot fractures of the femur, consist of 190 specimens, and is of great interest. Specimen 1042 A. M. M. shows a consolidated fracture of the femur, in which the ball entered a little below the great trochanter of the right femur, shattering the upper

third of the bone; this happened September 17th, 1862; on the 21st January, 1863, the fracture was firm. The man died 9th March following, with phthisis, which had begun about the time of consolidation.

We are told that the records contain scores of fatal cases of *Gun-shot Injuries of the Knee Joint*, treated by free incisions into the articulation. "Yet amputations for gun-shot injuries of the knee, that have reached the second period, are scarcely less disastrous," (p. 36.)

"Three patients have been photographed at the Army Medical Museum who had recovered without amputation after gun-shot injuries of the knee joint. Four or five additional cases appear upon the records. In scarcely any of these cases could it be asserted that the danger of consecutive disease of the knee joint was passed. The 'curious fact,' adverted to by Surgeon I. Moses, U. S. Vols.,* 'that more men had been discharged the service at that post [Louisville, Kentucky] who had received gun-shot wounds of the knee joint with recovery than when amputation of the thigh had been performed,' is directly contradicted by the official reports from that post," (p. 37.)

A number of drawings at the Army Medical Museum exhibit the course of balls directly in the track of the great vessels of the neck, and of the limbs, illustrating the resiliency of large arteries.

We are informed that—

"The number of *Sabre and Bayonet Wounds* that have come under treatment has been comparatively small; 105 cases of the former, and 143 of the latter comprise nearly all that have been reported for the first three years of the war. Of these wounds, two-thirds were received in action, and the remainder were inflicted by sentinels or patrols. There are 11 deaths from sword wounds recorded, and 6 from bayonet wounds. At the Army Medical Museum there are 9 specimens of sabre cuts of the cranium, a specimen of punctured fracture of the skull by a bayonet, and a preparation exhibiting a bayonet thrust through the stomach. From General Sheridan's campaign in the Shenandoah Valley, 25 sabre wounds are reported; and from the battle of Jonesborough, in Georgia, 30 bayonet wounds," (pp. 39-40.)

Of *Traumatic Tetanus*, 363 cases are all that have been reported during the war; 336 terminated fatally. Of the 27 recoveries the disease was of a chronic form in 23. "In the four remaining cases the symptoms were very grave. In two, recovery took place under the use of opiates and stimulants; in two, after amputation of the wounded part," (p. 42.)

"The great majority of the cases were treated by the free use of opium, conjoined with stimulants and concentrated nourishment. Chloroform inhalations were very generally employed during the par-

*American Journal of Medical Sciences, vol. xlvii., p. 341.

oxysms of spasmodic contraction. Subcutaneous injections of the salts of morphia and atropia were frequently used. Cathartics, quinia, camphor, cannabis indica, bromide of potassium, strychnia, belladonna, and aconite are mentioned among the remedies employed. Cups, blisters, turpentine stupes, and ice were among the applications made to the spine; and fomentations with opium or tobacco, were, in some cases, applied to the wound. Amputation, the division of nerves, and the extirpation of neuromata in stumps were the surgical measures sometimes employed. The results have not modified the conclusion of Romberg, that 'wherever tetanus puts on the acute form, no curative proceeding will avail, while in the milder and more tardy form, the most various remedies have been followed by cure.' The value of nicotine, of the Calabar bean, and of curare* as curative agents in tetanus was not tested," (p. 42.)

One case of recovery in the chronic form occurred within our observation, during the administration of large and frequent doses of the extractum cannabidis.

Autopsies were made in many cases, but with negative results, there being no microscopic examinations. Great congestion of the brain and spinal cord is "frequently mentioned," a condition on which the constant anatomical lesion of Rokitansky and Demme—proliferation of the connective tissue of the spinal cord and portions of the brain—is believed to depend. The influence of sudden vicissitudes of temperature, of unextracted balls and other foreign bodies, and matter confined under fasciæ, in developing this affection, is abundantly shown in the records.

Of 650 examined and recorded cases of *Secondary Hemorrhage from Gun-shot Wounds*, the termination was fatal in 330, or 51 per cent. It would appear—

"That, during the earlier part of the war, there were many surgeons who were not sufficiently impressed by the precepts of Bell and Guthrie, and who frequently treated secondary hemorrhage from gun-shot wounds by tying the main trunk at a distance from the wound, even when the bleeding occurred at a comparatively early period. Later in the war, however, it was the universal practice to endeavor to secure both ends of the bleeding vessel at the seat of injury, and some brilliant examples are recorded in which this was accomplished in wounds of the posterior tibial or popliteal, when limbs had become infiltrated and swollen, and the difficulties of the operations were immense, (p. 43.)

On the subject of *Pyæmia* we are informed that the histories of 754 cases are registered, the post-mortem observations accompanying a large proportion of the fatal ones, which numbered 719, or 95.35 per

* According to H. Demme, of 22 cases of traumatic tetanus treated by the latter agent, 8 recovered. See Schweiz. Zeitschrift für Heilkunde, ii., 356.

cent. These figures by no means represent the frequency of pyæmic poisoning, which was one of the great sources of mortality after amputations, "its victims being counted by thousands." The statistical reports on treatment "are adverse to the therapeutical utility of the sulphites and hyposulphites in this disease."

The form according to which all *Surgical Operations* are recorded on the registers of the office, though involving much labor, insures the highest attainable degree of statistical accuracy.

"The name and military description of the patient are given, the nature and date of his injury, an account of the operation, a notice of the local lesions which made it necessary, and of the constitutional condition of the patient at the period it was performed. A summary of the progress and after-treatment follows, and the result, if ascertained, the name of the operator, and the post-mortem appearances, when known, if the case terminated fatally. If the case furnished a pathological preparation to the Army Medical Museum, a reference is made to the number of the specimen on the catalogue," (p. 44.)

The returns of *Amputation at the Knee Joint* to October, 1864, give 132 cases, of which 52 got well and 64 died. Of 49 cases of primary amputation, 31 recovered and 16 died, a mortality percentage of 34.9. These results support the opinions of Malgaigne, Baudens, and Macleod, in opposition to Legouest. The objection to amputations at the knee joint, that the resulting stump is ill-adapted to the use of an artificial limb, is disposed of by the positive declaration of Hudson and other manufacturers, "that the stumps from the operation at the knee joint give a base of support far better than any possibly to be gained in thigh-stumps."

In 1597 terminated cases of *Amputations of the Thigh*, 64.43 per cent. died, which is within a fraction of the mortality after amputations of the thigh in the British army in the Crimea. In the French army 91.89 per cent. ended fatally. Of these 1597 amputations, 423 were known to be primary, and 638 intermediate or secondary. The ratio of mortality was 54.13 in the former, 74.76 in the latter.

There have been reported 23 *Amputations at the Hip Joint*, of which 9 were primary, with 2 recoveries, and 14 were secondary, with 3 recoveries.

"There seem to be but three conditions under which early amputation at the hip joint is admissible in military surgery, viz., when nearly the entire thigh is carried away by a large projectile, when the totality of the femur is destroyed by osteomyelitis, and, possibly, when, with comminution of the upper extremity of the femur, the femoral vessels are wounded. As to the method of operating, it may be observed that the anterior flap single procedure has of late been generally preferred," (p. 52.)

Nearly all of the cases of *Excision of the Shoulder Joint* reported during the war have been recorded.

"The percentage of mortality is 23.3 in primary cases, 38.59 in secondary cases, or a mean ratio of 32.48. The ratio in amputations at the shoulder joint is 39.24, a percentage of 6.76 in favor of excision. Of 36 cases of gun-shot fracture of the head of the humerus, selected as favorable cases for the expectant plan and treated without excision or amputation, 16 died, or 44.4 per cent., a ratio in favor of excision of 11.96 per cent," (p. 55.)*

The observation of Esmarch, that resection of the left shoulder gives less favorable results than that of the right, is not confirmed by the returns. The method commonly preferred was that by a single vertical incision, though some operators raised a V-shaped flap, and all endeavored to include the wound made by the ball in the incision. It is frequently mentioned that the long tendon of the biceps was preserved. Where the shaft of the humerus had been extensively shattered, five or six inches of the diaphysis, along with the head, were frequently removed, in spite of the prohibition of Guthrie, and with excellent results. In one remarkable case—

"After an incision of the head and upper third of the humerus, the remainder of the bone became necrosed, and was excised, together with the articular ends of the radius and ulna, and yet a limb was pre-

* Prof. A. G. Drachmann, in a recent communication to the Royal Medical Society of Copenhagen, states that the total number of resections of the shoulder and elbow joints, from gun-shot wounds, in the Danish army during the late war, was 30—16 of the shoulder and 14 of the elbow, of which 4 of the shoulder and 8 of the elbow came under his own observation. From these 12 cases, as well as from 12 others, (4 in or near the shoulder joint, and 8 in or near the elbow joint,) he is of the opinion: 1. That gun-shot wounds through the shoulder joint, with lesion of the respective bones composing the joint, may be healed, without resection, with a satisfactory result as to the future utility of the limb. 2. That gun-shot wounds in the immediate neighborhood of the shoulder and elbow joints, with injury of the bone, do not always cause the opening of the joint, and may be healed without limiting the usefulness of the limb. In our own experience, several remarkable cases of such injuries in the neighborhood of shoulder and elbow joints have made excellent recoveries without operative interference. The difficulty is to decide whether the joint is implicated or not.

Prof. Drachmann (*op. cit.*) believes, from his own experience, and that of other military surgeons, in the last Schleswig-Holstein war, that gun-shot wounds through the elbow joint, with lesions of the several component bones, may in general be healed with ankylosis of the joint, and with a result far better for the general utility of the arm, than that following resection, without ankylosis.—[Rev.]

served, which, with the aid of ingenious apparatus, is very useful," (p. 55.)

It appears that formal *Excisions of the Ankle Joint* were rarely successful.

Prior to the late rebellion there were but *seven* recorded cases of *Excision of the Knee Joint* for gun-shot injury, of which *two* were successful. Abstracts of 11 cases of this operation are given; of these 2 got well and 9 died.* Of one of the two recoveries the remark is made, "the success claimed is so extraordinary as to suggest some doubts of its authenticity." Three excisions of the patella are reported, the patients surviving the operations twelve, fifteen, and eighteen days, respectively. In another case the knee joint was laid open, and the fragments of a patella, shattered by a musket ball, were removed. The patient lived ten days. In two cases the head of the fibula was excised, and portions of the head of the tibia; both recovered. It does not clearly appear that the articulation was opened in either case.

There were 12 cases of *Excision of the Head of the Femur* on record previous to the war, with one success—Surgeon O'Leary's (68th British Foot) case, where the head and several inches of the shaft of the femur were excised for comminuted fracture of the great trochanter by a shell. [Med. and Surg. Hist. of the British Army in the Crimea, vol. ii., p. 378.] We have in this Report a tabular statement of 32 cases returned to the Surgeon General's office, of which 28 died, and 4 got well.

Though ample materials are on hand for arriving at definite conclusions respecting the value of *excisions in the continuity of the extremities*,

* In the essay on excision of the knee joint, submitted to the Council of the Royal College of Surgeons for the Jacksonian Prize, by the late Mr. P. C. Price, lately published, of 291 recorded cases of this operation, 78, or 1 in 3.7, terminated fatally. Among these, 238 had been performed by British surgeons, with 55 deaths, showing a mortality of 1 in 4.3. These calculations include 5 fatal cases in which amputation was performed after the failure of excision. In civil practice, Mr. Butcher, of Dublin, has reported five cases of excision of knee, all of which were successful. Mr. Fergusson, who revived the operation in 1850, says, (London *Lancet*, 1864, vol. ii., p. 33:) "I have now performed this operation *forty* times, and of these no less than *fifteen* have died, [37½ per cent.] My impression is that excision of the knee is, or should be, by proper treatment, as little destructive to life as amputation of the thigh."

M. Verneuil lately stated, at the Société Chirurgicale, (1865,) the operation has proved so fatal in the Paris hospitals, that it was nearly given up.—[Rev.]

compared with amputations, they have not yet been thoroughly analyzed; but so far as examined, they are, "on the whole, unfavorable to excisions in the continuity," (p. 76.)

Of the three cases of *Ligation of the Common Iliac*, all were fatal, the patients surviving two, four, and five days respectively. Two cases of ligation of the *Internal Iliac*, for secondary hemorrhage after gun-shot wounds, are recorded; both died. Two recoveries of the ligature of the *External Iliac* are mentioned.

"Of ligations of the subclavian, 35 cases are recorded. In all, the vessel was secured outside of the scaleni. In 16 cases, the operation was on the right, and in 14 on the left side; in five cases the particular vessel is not mentioned. The operation was performed in 13 cases for secondary hemorrhage after amputation of the shoulder-joint, with four recoveries. In two cases, it was done for primary, and in fifteen for secondary bleeding after gun-shot wounds, with injury of the axillary artery, with two recoveries. In two cases, the operation was performed for axillary aneurism. In two cases, with one recovery, it was required by secondary bleeding after excisions of the humerus, and in one case it was necessitated by a secondary hemorrhage after a gun-shot wound with injury of the subclavian," (p. 79.)

The vast influence exerted upon the results of the surgical practice of the war by scorbutic and malarial complications is thus referred to:

"It can not be doubted that the frequency of osteomyelitis after amputations, the proneness to suppurative inflammation in wounds of the head and splanchnic cavities, the tendency to the sloughing of flaps, the delay in the union of fractures or the healing of wounds, and the great prevalence of pyæmia, observed at certain periods and localities, were intimately connected with the morbid causes above mentioned, which had led, in many cases, to such an impairment of the general vitality of the men as greatly to diminish their ability to resist the effects of severe injuries," (p. 86.)

In 23,260 surgical operations performed on the field or in general hospitals, in which anæsthetics were used, chloroform was administered in 60 per cent., ether in 30 per cent., and in 10 per cent. a mixture of the two.

"At the general hospitals, the greater safety of ether as an anæsthetic was commonly conceded. It was often employed, and no fatal accident from its use has been reported. In the field operations, chloroform was almost exclusively used. The returns indicate that it was administered in not less than eighty thousand cases. In seven instances, fatal results have been ascribed with apparent fairness to its use," (p. 87.)

True *Contagious Hospital Gangrene* was comparatively limited.

The *Report*, modestly claiming to give "an idea of the nature and extent of the data for a *Medical History of the War*," embraces re-

marks upon the medical statistics of the several armies and general hospitals, fully prepared for the two first years; the memoirs and reports on the causes, symptoms, and treatment of the more important camp diseases; the series of medical and microscopical specimens in the Army Medical Museum; and the results of pathological studies on the basis of these collections. The exemption of our army from serious epidemics is attributed to an abundant commissariat, and also, quite drolly, to liberal medical supplies. The opinion is hazarded that "the mortality of our soldiers from disease has been far less than that of any other army in time of war." Still, the deaths "from disease were far more numerous than all the slain in all the battles, and all the wounded who have since died or are yet likely to die of the injuries received in the struggle;" and were more than five times as great, not counting deaths among prisoners of war or discharged soldiers, than of men of the same ages in civil life, "and were caused by diseases which are precisely those most under the control of hygienic means."

"The mortality of the armies of the United States from disease alone was 48.7 per 1000 of mean strength during the first year of the war, viz., the year ending June 30, 1862, and 65.2 per 1000 during the second year, viz., that ending June 30, 1863," (p. 92.)

It was considerably greater during the second year of the war than during the first. It varied, too, in the three great regions, Atlantic, Central, and Pacific, in which the troops operated. The Atlantic region included the North-Eastern and Middle Departments, the Army of the Potomac, the troops in North and South Carolina, and in Florida; the Pacific region embraced all west of the Rocky Mountains; and the Central Region contained the great base of the continent between the Appalachian and the Rocky Mountains.

On account of its great frequency and mortality, *Camp Fever* was, during the two years under consideration, the most important of the diseases of the army.

"In a general way, it may be said that each year about one-quarter of the men suffered from some form of the fever, and that the deaths amounted to about two per cent. of the strength. The whole number of deaths from these fevers during the first year was nearly one-half the total mortality from disease; during the second year, owing to the increased mortality from other diseases, and especially from diarrhœa and dysentery, only about one-third the total mortality from disease, though still maintaining nearly the same ratio to strength," (p. 110.)

Under the general designation of *Camp* or *Typho-Malarial Fever* are included all those cases which were reported, "during the first year of

the war, under the heads of Typhus, Typhoid, Common Continued, and Remittent."

We may venture to express a regret at the adoption of this nomenclature, and a fear that it will prove as confusing as the old comprehensive title of "Common Continued Fever," which included so many types of fever most diverse in their nature, and will hinder the investigation of the pathogeny of the several specific fevers which prevailed in our army, and the determining of the varied conditions under which they were generated and propagated.

Modern pathology recognizes certain distinct varieties of idiopathic fevers, whose special nature is well defined, whose natural history has been thoroughly studied on fixed scientific principles, and whose ætic poisons have specific vital attributes peculiar to themselves, and different *local foci*. Bayle remarks: "The determination of specific characters is what is most essential in nosology." "Words," says Sauvages, "are good only in respect to their signification." We consider the correctness of this classification of fevers open to question, as not abreast of the time, and as necessarily favoring the notion of identity between diseases which have no community of origin. While any intention, by this grouping, "to express a doubt as to the propriety of regarding typhus, typhoid, or enteric and remittent fevers as distinct affections," is expressly disclaimed; and we are told, rather oddly, too, that "the enteric lesion characteristic of typhoid fever enables a ready distinction to be effected between it and genuine typhus or true remittent, *on the autopsy at least*;" and that "undoubtedly cases of simple enteric and simple remittent fevers did occur;" yet we have the adoption of the "general name," typho-malarial fever, and all the fevers of the army described under its three varieties—malarial, scorbutic, and enteric—the following reasons being assigned for this violent and unnatural arrangement:

"As the diseases have occurred in our army during the present war, the phenomena of these two [*three* have just been named, typhoid, genuine typhus, and remittent] affections have continually complicated each other in the same patient; so that, in fact, the enteric fevers have broken out among men campaigning in a malarial region, with constitutions more or less thoroughly impregnated with the malarial poison; the remittents among soldiers peculiarly prone by their exposures and mode of life to enteric disease; and both have occurred, almost without exception, in men whose health has been more or less modified by camp diet, and who were therefore suffering in some degree from a condition best characterized as the scorbutic taint. These three modifying conditions or tendencies, each of which, acting alone, might produce simple enteric fever, periodic fever, or scurvy, when acting

simultaneously produce mixed types of disease that vary infinitely in accordance with the predominance of one or another of the three sets of determining conditions," (p. 109.)

We have always regarded the term typho-malarial* as unfortunate, and liable to lead to confusion and error. We fear that the author of the Report has been unduly biased in favor of the bantling he stands sponsor for. It was at his suggestion, we are informed, (p. 109,) that this "general name was adopted into the statistical nomenclature of the monthly reports of sick and wounded." Had this not been done so early as June, 1862, is it not probable that subsequent enlarged clinical experience may have modified his views?

We recognize, to their fullest extent, all the modifying influences enumerated, but we hold that in every case they were engrafted on some specific admitted type of fever, and were accessory phenomena. We think the objections that we have suggested to this nomenclature of fevers are sustained, when we come to the imperfect summaries of the symptoms, and the scrupulously accurate descriptions of the recent anatomical characters of each form. They strengthen our belief that a plurality of fevers prevailed in our armies during the rebellion, and prove that while they were frequently, and under certain circumstances, constantly, allied by certain common characters, they were not the less separable by peculiar and distinctive traits. We saw regularly, for a series of years, every autumn, cases of typhoid fever where the proper symptoms were much modified, and often greatly masked by pallidal poisoning, but in no case were those symptoms so occult as to prevent a correct diagnosis from being made. And we have seen the same state of things constantly among the fever cases of our armies. In fairness, it must be admitted that the fever which was so prevalent in the Army of the Potomac, when it was floundering in the marshes of the Chickahominy, was of a puzzling type, so far as its easy assignment to any of the known forms of fever which had existed in this country during the past thirty years, at least in the Northern and Middle States. In men saturated with malaria, exhausted by long exertion and insufficient rest, imperfectly nourished, exposed to the action of animal effluvia from the decaying bodies of both men and brutes, and daily drinking water impregnated with the products of common putrefaction—all tending to lower the energies of the nervous system and corrupt the blood—there was produced a dis-

* Why was the scorbutic element refused fellowship with the pallidal and idio-miasmatic elements? It has, and properly so, no mean pathogenic part assigned to it.

ease in which the combined action of pythogenetic, palludal and scorbutic causes must be acknowledged. But there was really nothing novel in either its symptoms or pathology.

This form of fever, of remitting type at the outset, with abdominal tenderness, diarrhœa, enlarged spleen, and often cerebral and pulmonary complications, (we summarize the imperfect description of the Report,) is designated the "malarial form of typho-malarial fever." Had the use of the term typho-malarial been restricted to this variety of idiopathic fever, no serious objection could have been made to it.

"The characteristic lesion is enlargement of the solitary follicles of the small intestine, and especially of the ileum. Thickening of Peyer's patches may be quite absent, or may be present to a variable extent. All degrees of enlargement have been noticed, from the slightest change to cases in which the follicle attains the size of a pea. The most characteristic specimens may be thus described: in the fresh intestine, the ileum presents patches of deep congestion of variable extent; the solitary follicles, enlarged to the size of large pin-heads, are frequently black with pigment deposits. The Peyer's patches, sometimes quite healthy, are more generally the seat of pigment deposits in the individual follicles composing the patch, which appears of a gray color, dotted over with blackish points, presenting a resemblance to the freshly-shaven chin. The name 'shaven-beard appearance' has been quite currently bestowed upon this condition. In other cases, the Peyer's patches are somewhat thickened, and occasionally as much so as in ordinary cases of enteric fever," (p. 140.)

The second and rarer form of the disease, styled "scorbutic form of typho-malarial fever," is described as "marked by the extremely adynamic character of the symptoms, by petechiæ, at times even by the characteristic scorbutic lesions of the mouth, by hemmorrhage from the bowels, and other hemmorrhages which complicate the disorder and often prove fatal." In the specimens, as first received at the Museum—

"The ileum presents intense reddish-black patches of congestion, which sometimes extend throughout its whole length. The patches of Peyer are converted into livid, blackish, pulp-like sloughs, which are often remarkable for their size and fungoid appearance. Petechia-like blotches in the mucous membrane of the colon, the small intestine, and the stomach are of frequent occurrence. Similar discolorations are at times observed in other organs. The cadaver often presents petechiæ on the external surface of the body and scorbutic alterations of the mouth," (pp. 140, 141.)

In the "enteric form of typho-malarial fever," we are told, in somewhat contradictory language, that the

"Specimens of the third group are quite identical with those obtained from the typhoid or enteric fever of civil life, and the cases in many instances are undoubtedly that affection in its ordinary form. As it occurred among the troops, however, the course of the disease was generally more or less modified by the influence on the soldier of malaria, or of the scorbutic taint, or both," (p. 41.)

A comparison of the reports from the three great regions, shows Camp Fever to have been far more frequent in the Atlantic and Central regions than in the Pacific.

"In the Atlantic region, during both years, the number of cases was somewhat less than one-fourth the strength; the deaths for each year about seventeen per 1000 of strength. In the Central region, the ratio of both cases and deaths was much greater during the first year than the second. During the first, the cases amounted to nearly one-third the strength, the deaths to about thirty-two per 1000 of strength. During the second year, the cases were somewhat less than one-fourth the strength, the deaths twenty-four per 1000 of strength. In the Pacific region, the cases for each year amounted only to between seventy and eighty per 1000 of strength, the deaths somewhat over one per 1000 during the first year, somewhat less than one per 1000 during the second," (pp. 110-111.)

The modifying influence of region on the mortality is more strikingly shown when, instead of comparing the deaths with strength, they are compared with the number of cases, for it is then seen that "the disease is not only more frequent, but more fatal, in proportion to the number of cases, in the Central region than in the Atlantic, and in this more than in the Pacific."

The occurrence of true *Typhus*—the *Pestis Bellica* and scourge of all large armies, from the siege of Syracuse to that of Sebastopol—in our armies, during the late rebellion, has been doubted, but there is sufficient evidence to show that there was a limited number of cases "in connection with overcrowded and ill policed camps," and especially among "those of our soldiers who were detained as prisoners in the enemy's hands."

Interesting materials on the subject of *Cerebro-Spinal Meningitis*, called also, improperly, we hold, Spotted Fever, have been contributed, but the fact is only mentioned.

Yellow Fever made its appearance at Key West, Florida, in 1862, and subsequently, in the same autumn, at Hilton Head, South Carolina.

"The outbreak was limited to a few hundred cases, and the deaths to a hundred. In both places there was the most decisive evidence that the disease was imported in consequence of the neglect or viola-

tion of quarantine regulations. The fear that yellow fever would prove a terrible obstacle to the operation of our troops in the Southern States has proved wholly unfounded. It may here be mentioned that the only subsequent outbreak of importance which has occurred up to the date of writing, was the epidemic at Newbern, North Carolina, in the summer of 1864. But even here the mortality, so far as our troops were concerned, was limited to a few hundred men. In the case of this outbreak, Surgeon D. W. Hand, U. S. Vols., Medical Director of the Department, expresses the belief that the fever was not imported, but that it originated on the spot in consequence of the neglect of hygienic precaution by the citizens of the place and by the refugees who had made it an asylum," (p. 113.)

The severe visitation of Key West, in the summer of 1864, seems to have been overlooked, and there is no doubt that there were cases of Yellow Fever at New Orleans during the autumn of the same year.

The total number of all forms of *Intermittent Fever* reported for the two years was 262,807; the number of deaths, including the so-called congestive form, for the same period, was 1,788.

"Besides developing intermittent fever [does remitting fever own any other cause?—REV.] and complicating other diseases, such as camp fever and diarrhœa, the malarial influence manifests itself with considerable frequency among troops exposed to its action by the development of a peculiar form of anæmia, which may be designated Chronic Malarial Poisoning. This condition, attended usually with enlargement of the spleen and frequently with an increase in the number of the white corpuscles of the blood, manifests itself externally by languor, feebleness, and pallor, attended commonly with neuralgic pains, and, as it actually occurred among our troops, often complicated by slight scorbutic symptoms. Attacks of fever, pneumonia, or other acute diseases occurring among patients in this condition are peculiarly apt to prove fatal. A yellowish complexion is a frequent phenomenon in the form of anæmia here referred to, and often amounts to decided jaundice.

"Mild epidemics of jaundice, running a course of from two to six or eight weeks, and usually terminating in recovery, have also been of frequent occurrence among our troops in malarial regions. That this form of the affection also stands related to the malarial poison, is shown by the fact that, as a general rule, it was most common in those localities in which intermittents were most frequent, (p. 117.)

Diarrhœa and *Dysentery*, disorders of great frequency, being more than one-fourth of all the cases of disease, and, next after Camp Fever, the chief cause of mortality from disease, we find reported under four heads—acute diarrhœa, chronic diarrhœa, acute dysentery, and chronic dysentery. The terms "diarrhœa" and "dysentery" appear to have been loosely used in the Reports. "The disease most generally called

chronic diarrhœa, was, in fact, usually an affection of the large intestine, which was thickened, softened, and often ulcerated. The term dysentery would have been more exact, and was bestowed by many surgeons upon the same affection which others called diarrhœa. Hence, it has been thought advisable, in considering the figures, to group together all cases reported under these heads," (pp. 117-118.) The annual number of cases for the whole army was greater than three-fourths of the mean strength. The total number of cases reported during the first year was 215,214, with 1194 deaths; during the second year, 510,461 cases, and 10,366 deaths—total, 725,675 cases, and 11,560 deaths. Taking the total of the several forms—

"It will be seen that the ratio of cases was 765 per 1000 of mean strength during the first year, and 852 per 1000 for the second; so that considerably more than three-fourths of the whole strength was attacked each year. The mortality was 4 per 1000 of strength during the first year, and 16 during the second; the disease being just four times more fatal during the second year than the first," (p. 118.)

The greatly increased mortality in the second year will be found, on an examination of the tabulated statistics, to be explained by the comparatively mild form of the acute variety, and the increasing severity of the chronic. The deaths from acute diarrhœa and dysentery in the first year were 1 to every 331 cases, and in the second year, 1 to every 245; while in the chronic forms the mortality increased from 1 death in every 30 cases, in the first year, to 1 in every 8 in the second.

"Like camp fever and intermittents, diarrhœa and dysentery were most frequent in the Central region; less so in the Atlantic, and least in the Pacific region. In the Central region, the cases were more numerous than the strength during the first year, and nearly equal to the strength during the second; in the Atlantic, they were more than half the strength during the first year, and more than three-quarters during the second; in the Pacific region, during each year somewhat over one-quarter the strength. The differences between the ratio of mortality to strength, in the three regions, were still more striking; in the Central region the mortality was 9 per 1000 of mean strength during the first year, 23 per 1000 during the second; in the Atlantic, 1 per 1000 during the first year, 9 per 1000 during the second; in the Pacific region less than 1 per 1000 during each year," (p. 119.)

They were, by far, most frequent in the summer and autumnal months.

As in the case of camp fever, it can not but be regretted that the acute and chronic forms of diarrhœa and dysentery have been grouped together under one head in the statistical returns. Still, it will not

materially damage the practical study of the causes, symptoms, nature, and treatment of that scourge of our armies, camp or chronic diarrhœa, as it was almost universally called. Although no attempt is made in the Report to present any analysis of the vast amount of material that has been contributed on this important disease, some general views of great interest are hazarded, which we regret we can not present more fully to our readers, owing to the great length this article has already reached. We will endeavor to briefly mention some of the salient points. The causation of chronic diarrhœa is to be found in no one condition, but the long continued and co-operative action of certain influences, chief amongst which are the scorbutic taint, due to camp diet, malarial poisoning, the filth and overcrowding of camp and barracks, excessive and prolonged heat, physical fatigue and exhaustion during active campaigns, and impure water. "Whether there has ever existed, in addition to these intelligible conditions, any specific causative momentum deserving the designation of epidemic influence, is a grave question, which receives no affirmative reply from any experience reported during this war." There is no "specific cause, or set of causes, different from those which induce the acute form." Frequently, perhaps usually, repeated attacks of acute diarrhœa preceded the more serious and continued disorder; hence the presumption is warranted "that a certain length of time is required before the influences to which a soldier is exposed culminate in chronic diarrhœa."

To the statement that "among its most striking phenomena may be mentioned the usual absence of fever throughout the greater part of its course," we must put in a demurrer. If Galen's definition of fever, "*calor præter naturam*" is correct—and it can not be disputed that of all the clauses and phrases in the many definitions of fever attempted by systematic writers, it is the only one whose accuracy is unimpeachable—we are confident that febrile phenomena would have been found present in every case of chronic diarrhœa in our armies, had the amount of preternatural heat in each case been ascertained by accurate thermometric measurement. The "*dry, harsh condition of the skin*" is admitted, as well as the "*extreme emaciation*"—the latter symptom being due to *increased amount of tissue change*, another constant phenomenon of fever, and one exhibiting a certain co-relation to, and association with, morbid development of the heat of the body. It is to be regretted that the products excreted by the lungs, skin, and kidneys, as well as by the bowels, were not the subject of frequent examination and investigation in this disorder, and that it was not ascertained in what organs increased or diminished elimination was the rule. Our own

observations have inclined us to entertain the belief that camp diarrhœa was a *chronic autophagic disease*, induced mainly by the causes heretofore enumerated—causes, in themselves, affecting and impairing the proper nutrition of the body, inducing a condition of gradual and slowly continuous semi-starvation, the body literally feeding on itself, and that death happens in a state of extreme debility and exhaustion, except in the exceptional cases where some acute accidental complication kills the patient. These intercurrent affections were, we think, much less frequent than the reporter claims, the supervening fever and acute dysenteric symptoms being natural terminal phenomena of prolonged autophagism. It is not to be denied, however, that a patient suffering from chronic diarrhœa may be attacked with camp fever or acute dysentery, and the characteristic lesions of these diseases be found variously and curiously combined.

The pathological anatomy of camp diarrhœa is illustrated by over 200 specimens in the Army Medical Museum, arranged in four groups: 1st, examples of follicular ulceration of the colon, with thickening of the intestinal coats, which ulcers extend, by burrowing in the submucous connective tissue, until, in extreme cases, the mucous membrane of the colon is destroyed by vast erosions. In some cases the surface of the gut is more or less coated with a yellowish, or greenish yellow, pseudo-membranous layer, similar to the membrane formed in the air-passages in diphtheria, and is generally found after the sudden super-vention of symptoms of acute dysentery. In a few cases the small intestine is implicated, the ileum being more or less thickened, particularly near the ileo-cœcal valve, with ulcers of variable size which appear to have their origin in the solitary follicles.

With respect to the treatment of chronic diarrhœa, we are told, the whole range of vegetable and mineral tonics, and alteratives, and astringents have been employed with variable success, and subnitrate of bismuth, strychnia, and arsenic are particularly named.

"The utter failure of these, or indeed any therapeutic agents, to command general confidence, or to come into general use, will show how subordinate their effect is to be regarded to that of proper dietetic and climatic conditions," (p. 126.)

All medical men who have had large experience in treating this disease, will admit the impotency of drugs and the value of diet and climate. The latter is an essential element in the treatment. Its influence is absolute. Without it all other means are but palliative and temporary; it is the only one which is followed by abiding results. The value of change of climate in the treatment of chronic diarrhœa

was recognized in the Mexican war, and during the late rebellion the Medical Department "was fully alive to the advantages to be derived from that source, and availed itself of them as far as, at the time, with a full knowledge of all the circumstances of the case, it was believed to be practicable." Patients suffering from chronic diarrhœa in the middle and southern districts of the Atlantic region were transported to the hospitals in the State of Vermont, and with success. In the West such cases were sent to hospitals in high northern regions, as at Keokuk, Ia., Madison, Wis., Chicago and Detroit.

As "considerable confusion appears to have existed as to the precise signification and limits of the terms catarrh, epidemic catarrh, and acute bronchitis, precisely similar cases being reported by different surgeons under each of these heads," and believing that the causes determining inflammatory affections of the several portions of the respiratory apparatus are intimately allied, it was thought "advisable to bring together, in a single group, all the disorders of this class," designating them *Inflammatory Diseases of the Respiratory Organs*, and including all the cases reported as epidemic catarrh, catarrh, acute and chronic bronchitis, laryngitis, pleurisy, and pneumonia; again this violent and artificial grouping may have been necessary, but is not, for obvious reasons, the less regrettable.

"The total number of cases during the first year amounted to more than one-half the mean strength; during the second year, however, to not much more than one-quarter of the strength. The deaths were between 8 and 9 per 1000 of strength during each year," (p. 128.)

Unlike camp fever and diarrhœa, this group of diseases happened with nearly equal frequency in the three regions. The proportion of mortality to strength, however, followed the same general law as the other camp diseases, being most frequent in the Central, and least so in the Pacific region. The proportion of deaths to cases was likewise greater in the Central than in the Atlantic, and in this than in the Pacific.

"In the Atlantic region there was one death to every 123 cases during the first year, one to every 71 during the second; in the Central region one to every 31 cases during the first year, one to every 18 during the second; in the Pacific region one to every 291 cases during the first year, one to every 211 during the second. The average for all regions and both years was one death to every 38 cases," (p. 130.)

The greatest number of deaths from the inflammatory affections of the respiratory organs were reported under the head of Pneumonia. Out of a total of 8090 deaths from respiratory diseases, 7091 are due to this cause. The experience of the two years gives a mortality of

one death to every seven (6.8) cases in the Atlantic region, and one to every four (3.8) in the Central; "and this proportion is so great, as compared with the results in modern civil hospitals, as to direct attention to the general want of success which appears to have attended the treatment of this disorder," (p. 131.)

This startling proportion of deaths to cases was, to a certain extent, due to the then prevalent types of the disease, called, in the Report, "Typhoid Pneumonia," and "Adynamic Pleuro-Pneumonia." The mortality from pneumonia in the British Army, in the Crimea, was one death in every 3.6 cases. We are surprised to find not one word respecting capillary bronchitis, which was not infrequent among the white troops in certain regions, and very common among the colored soldiers. It was usually confounded with typhoid pneumonia.

Respecting *Scurvy*, we are informed that the amount reported was comparatively small.

"1328 cases and 9 deaths for the first year; 7395 cases and 90 deaths for the second. To this may probably be added the greater part of the 304 cases and 31 deaths of purpura reported during the second year. This extremely small number of cases of scurvy is unparalleled in the history of armies, being but 5 per 1000 of mean strength for the first year and 13 for the second. It undoubtedly stands related to the quantity and comparatively good quality of the army ration,—to the immense supplies of antiscorbutics, of medical stores and comforts issued to the men by the Government, and to the large pay of the private soldier, which is very many times greater than in any other army in the world, and which, in part at least, was often spent at the sutler's on pickles, apples, pies containing dried fruit, etc. From all these sources, ours have undoubtedly been the best fed soldiers in the world," (p. 134.)

This is a satisfactory statement to read, but how far it is really supported by facts, we leave those to judge whose opportunities were large in our armies, and to the Report itself which signally contradicts it. Besides it being insisted on that scurvy was a constant complication in camp fever and camp diarrhœa, we find, a few lines after, this most extraordinary assertion—"a scorbutic taint, more or less pronounced, was a prominent phenomenon in most of the diseases of the war," (p. 134.)

Again: "The scorbutic taint manifested itself very generally in the form of rheumatic pains in the back and limbs, associated with the scorbutic, clay-like appearance of the skin, sometimes even with sponginess of the gums, much more rarely with petechiæ, scorbutic discolorations about the flexure of the knee, etc. Most of the physicians called upon to treat these cases, having had in their previous private practice little experience with scurvy, reported them as rheumatism, lumbago, or neuralgia," (p. 134.)

Does not this admission account for the amount of scurvy reported

being "comparatively small?" The records of the two last years of the war will, we suspect, show a very different state of things, and lead the Reporter to qualify somewhat his inordinate laudation of the army ration.

Of *Tubercular Diseases* there were 8.9 cases per 1000 of mean strength for the first year, 9.3 for the second. The deaths were 1 to every 4.5 cases during the first year, 1 to every 2.7 during the second.

Notwithstanding the length of this notice, we have been able to give our readers only an imperfect notion of these Reports. Many matters of interest we have not been able to touch upon, from want of space at our disposal. They are sterling productions, and singularly free from blemishes, defects, or shortcomings. It is indeed difficult to measure the praise that is honestly due their authors, who both possess fitness for the perilous and laborious tasks assigned them. The "Surgical Report" is marked by clearness of language, precision of statement, and a generally quiet, unassuming tone. Dr. ORIS has been remarkably cautious in hazarding premature conclusions from statistics which are daily augmenting, and tending towards completion. Dr. WOODWARD, in the "Medical Report," shows great acuteness and industry in his intelligent tabulation of the accumulated data, by which he has been able to give numerical expression to the sick and death rates, and the frequency and mortality of certain diseases; and patient research in his valuable and original investigations in micro-pathology. He seems fully to appreciate the practical aspect of his subject.

The Messrs. Lippincott have produced the volume in a most creditable manner, leaving nothing to be desired in the way of paper and typography. The several lithographs and numerous wood illustrations are well executed.

In conclusion, it must be remembered that, following the example set by the Medical Department of the British Army after the Crimean war, both the Medico-Chirurgical History of the late war and the Army Medical Museum originated with Dr. W. A. HAMMOND, when Surgeon-General, and their inauguration was amongst his earliest official acts. Before his enforced retirement they had both made substantial progress. Nor in this connection must be forgotten the claims and merits of a gentleman who created, organized, and, for more than two years, had entire charge of the Museum; who was the Surgical Historian of the War during that time, and who, in the discharge of every duty he was assigned to while in the service—whether in the field, the general hospital, or the Medical Bureau—exhibited conscience, zeal and capacity—Dr. J. H. BRINTON, of Philadelphia.

Rhinoscopy and Laryngoscopy. Their Value in Practical Medicine.

By DR. FREDERICH SEMELEDER, Physician in Ordinary to His Majesty the Emperor of Mexico; Member of the Royal Medical Society of Vienna, and of the Medical Society of the Pantheon in Paris; formerly Member of the Medical Faculty of the University of Vienna, and Surgeon to the Branch Hospital of Gumpendorf. Translated from the German, by EDWARD T. CASWELL, M.D. With Wood-cuts, and two Chromo-Lithographic Plates. New York: William Wood & Co., 1866. pp. 191.

The work before us consists of two monographs on Rhinoscopy and Laryngoscopy, respectively, with an Appendix containing the record of two cases of Extirpation of Polypi in the Larynx.

It is from the pen of one of the most accomplished and successful laryngoscopists of Germany, who is also a general surgeon of no ordinary rank, as was evinced to those who had the good fortune to listen to the private instruction of Dr. Semeleder, or visit his wards in the hospital at Gumpendorf, a suburb of Vienna.

The translator, Dr. Caswell, of Providence, has enjoyed both the instruction and friendship of Dr. Semeleder, and "his task has been performed with additional pleasure and zeal from the recollection of the many happy hours passed with his good friend and faithful instructor, the author."

In the author's preface it is stated that the work is "intended to present facts, and to be of practical use," and if the reader's judgment coincide with ours, he will acknowledge that this end has been attained. Rhinoscopy, its history and performance, is quite fully given in the sixty-five pages allotted to it. The pathological cases, showing what may be done by this means in the way of a careful and accurate diagnosis, are interesting. The difficulties preventing the successful use of the rhinoscope are freely and frankly acknowledged. The author says: "Even now, when so great a number of examinations have been made, we can not determine upon a comparative percentage (of success); for here the practice and the adroitness of the observer are still more important than on the examination of the larynx." These difficulties, which can not be urged against laryngoscopy, which is an art practiced with comparative ease, will prevent the busy practitioner from accomplishing much with the limited time he may have at his command for this mode of examination; but here and there in the profession are those who stop at no difficulties in scientific investigation. Czermak, Störk, Voltolini, and Semeleder, of Germany, and, we

may say, Simrock, of New York, have shown us that rhinoscopic examinations may contribute very much to our knowledge of diseases of the pharynx, Schneiderian membrane, and eustachian tubes. The part of this work devoted to rhinoscopy having much in it that is new to the American reader, will, we are confident, secure the careful reading and thought which it deserves.

Laryngoscopy has been so zealously encouraged among us, and we have so many faithful workers in this field of our art, that this part of Dr. Semeleder's book will perhaps not be found to contain so much that is new. Yet in the sections on the Physiology of the Larynx, there is much which strikes us as peculiar and interesting. Dr. Semeleder claims that "the physiology of the formation of the human voice has been greatly elucidated by the laryngoscope, and especially by the labors of Garcia, Czermak, Moura-Bataille, and Merkel." He says: "The actual requisites for the production of tone are approximation of the arytenoid cartilages, (a progressive closure of the glottis from before, backwards,) tension of the vocal chords, and finally a current of air of a certain intensity and rapidity. Alterations of one or more of these qualifications disturb the formation of the voice, and produce hoarseness, or loss of voice, which are for us the same phenomena, differing only in intensity." Again, as to speech: "If we consider for one moment speech in itself, we must perceive that what we generally designate as speech, is actually made up of two parts, of a succession of more or less musical notes, vowels and liquids, and of a series of sounds arising from the closure formed at different parts of the mouth, as at the lips, the teeth, the tongue, the gums. If this closure is suddenly interrupted, or if it exists under such conditions that the current of air produces a rubbing sound, or that the parts forming the obstacle vibrate, then we have the various consonants of different languages." There is quite a complete account of the affections of the larynx, diseases of the mucous membrane of the larynx, of the sub-mucous connective tissue, secondary diseases of the larynx, with illustrative cases. In the two pages devoted to the negative results of laryngeal examination, an interesting case occurs. A boy was brought to the author at the Gumpendorf hospital, who was said to have lost the power of speech. The family were greatly excited, and the mother stated that the patient had spasmodic twitchings in the arms and legs; that he learned with difficulty. The face was greatly reddened, movements of the heart very violent, a systolic murmur, and further nothing abnormal. The laryngoscopic examination showed the larynx to be normal, and vocal chords readily

movable. On seeing this, and that the boy grew very red at the cross-questioning, malingering was diagnosticated; and after a severe scolding and a promise of secrecy on the part of the surgeon, the patient, suddenly restored, said that he had been beaten at school for having learned nothing, and hoped by this tragic comedy to escape instruction for the future.

It is also added, as a negative result of laryngeal examination, that catarrh of the pharynx has been very often found in patients who feared that they were suffering from laryngeal phthisis.

As to means of examination, methods of illumination, etc., in Rhinoscopy or Laryngoscopy, Dr Semeleder is not over-strenuous as to any one method or set of appliances, preferring, however, his own illuminating spectacles, (which are sold by the instrument makers of New York,) and a good moderator lamp, or sun-light converged, and the ordinary glass or steel mirrors, which should be thick, in order to remain warm longer. The bibliographic list is a very valuable part of the book, and Dr. Semeleder's original one has been made more complete by the translator. In our somewhat careful reading of this book we have found much that is of practical value, and we believe this will be the verdict of the professional public to whom it comes. We should have been glad to see a little more fullness in description occasionally; and a few more engravings, showing the exact shape of instruments and the manipulations necessary in their use, would have materially increased the value of the work.

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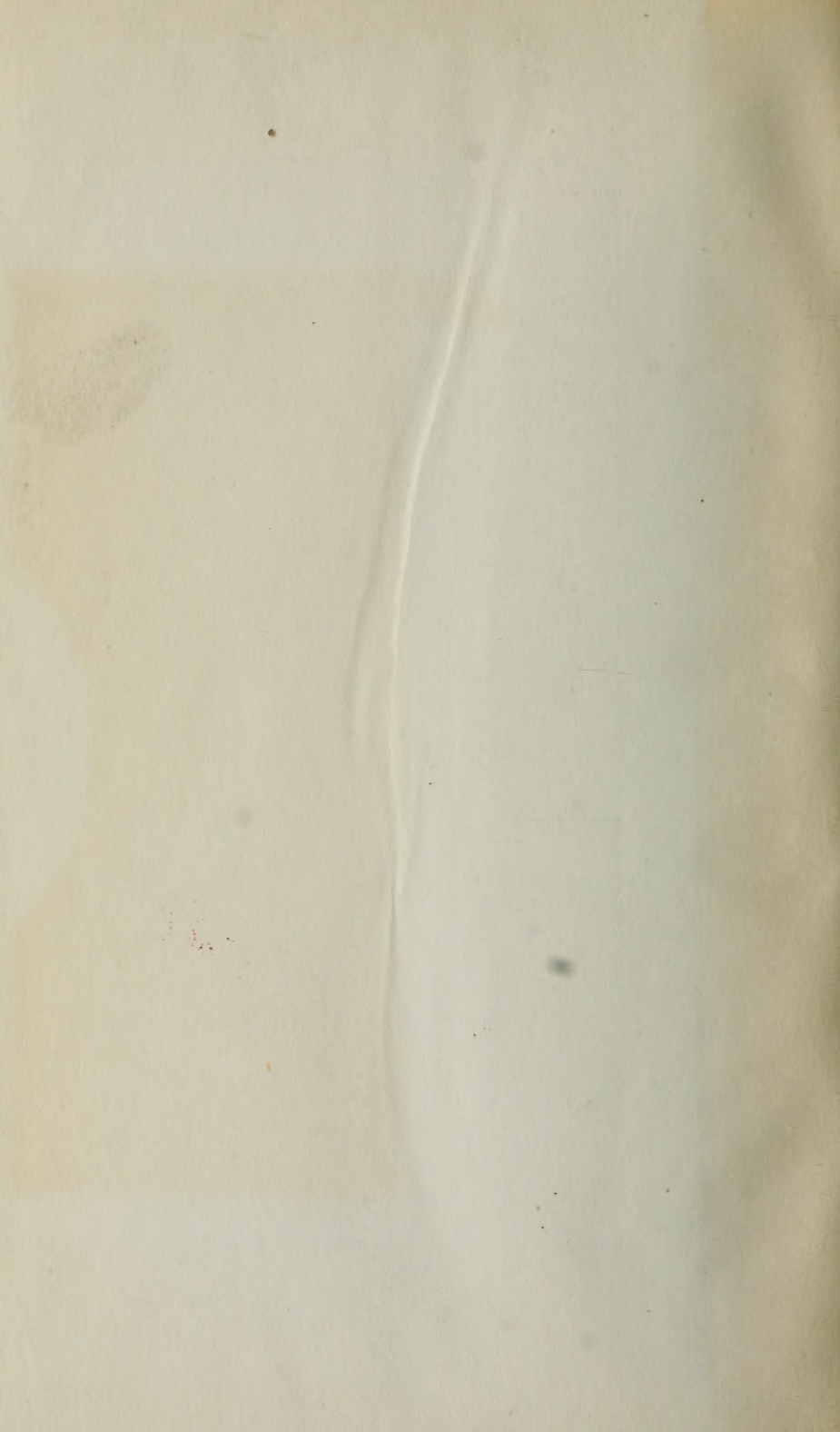
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